

THE
PSYCHOLOGICAL BULLETIN

A FEW TRENDS IN MODERN PSYCHIATRY.¹

I.

Psychiatry has had a peculiar fate owing to the great difficulty in finding a proper place in the sphere of experience for what had been so one-sidedly elaborated as soul and mind for religious-philosophical schemes before man became an object of biological investigation. Psychologists always took a certain interest in the peculiar phenomena and frequently exerted a strong direct or indirect influence on the presentations of the facts of observation. But the lack of actual experience on the side of the mental philosopher, and the lack of training in psychology on the part of most physicians, were two factors which added considerable difficulty to the anyhow complex problem. To this day further reasons conspired so as to give psychiatry a poorly defined position especially in Anglo-Saxon countries; and in order to bring about a fair understanding between physicians and psychologists a review of orientation of the principal issues of modern psychology naturally suggested itself.

Before psychology was one of the biological sciences, psychiatry shared its manifold vicissitudes.

Kant declared that the diseases of the mind were a topic for the philosopher, not one for the physician. Since then, a remarkable transformation has taken place, both in philosophy and in medicine. The extreme faith in verbal wisdom and in 'the absolute' had to be pushed to the extreme by the German philosophers, before a really powerful reaction could take place. An essentially speculative philosophy of a Hegel had to show the futility of a method which did not shape thought according to actual experience, but, on the contrary, created a set of methods of thought which it forced on the facts, without any consideration for the pluralism or multiplicity of experience.

¹ This number has been prepared under the editorial care of Dr. Adolf Meyer.

The pendulum of interest was bound to swing back in the direction of higher appreciation of simple experience, perhaps again to an extreme, but merely to pass into a much better balanced phase of peaceful and fruitful collaboration of empiricism and critical methods of thought and research. After a period of the crude materialism in medical circles of the middle of the last century, the creation of a sounder psychology led safely and strongly to a sounder relation between philosophy and medicine and science. Their interdependence became clearer again. The conflict of science and religion, in which philosophy had to play a part, gradually lost its importance, although, curiously enough, again in Germany, a certain faction of the clergy not more than ten years ago seriously endeavored to force the physicians out of psychiatry, and to claim the diseases of the mind for the moral and religious adviser. These extremes must seem preposterous to the Anglo-Saxon; but they deserve the attention of the student of psychiatric development.

In the Anglo-Saxon countries psychiatry was an essentially practical proposition. The question was how to care for those whom the law had stamped as incompetent to care for themselves, and who were considered unsafe in the hands of even their own family or immediate community. The business instinct of the race, economic issues, and the love for large enterprises, led to the foundation of huge hospitals, usually sparingly provided with physicians, and out of touch with the interests of learning and research of medical centers. Psychiatry was taught but little, and chiefly *ex cathedra*. It was too difficult a topic to be dealt with in the same way as many other diseases. As there was no time in the medical curriculum to teach the methods of 'theory and practice' of psychiatry in practical work, as surgery and the theory and practice of medicine are taught in actual work, simplicity of presentation and plausibility of statement became more important in lectures and treatises than an unbiased and painstaking consideration of actual experience. There was more concern about classification than about investigation. Psychiatry worked out more statistics than methods of recording the actual facts, and to this day the illusion is apt to crop out that a systematic scheme of nomenclature and a classification of all the cases should first be demanded as an ideal achievement, while hardly any adequate effort is made to ensure sufficient value and accuracy of the facts to be classified.

The psychiatric literature in the English language has nevertheless matured very creditable works, partly direct elaborations of personal experience, partly elaborations along the lines of French and German

pioneers. For some reason there is, however, a rather striking uniformity and an absence of definite schools of research which not only would bring out stimulating contrasts, but also would prompt individuals to concentration on specially fruitful topics, in preference to endless generalities. We find especially prominent therapeutic ambitions, but unfortunately too often in fields in which the object of treatment is bound to be vague and hypothetical. This explains why a high development in many of the practical measures goes side by side with a striking traditionalism and nominalism, and why the psychiatry of most writers cannot escape the comment that it is much more concerned with the adaptation of the mass of facts to a limited traditional set of terms than with a free and unprejudiced analysis and progressive grouping of the facts as they are. This is not as severe a criticism as it may seem at first sight. Under the formal shell, a sound practical sense exists, just as there is much sound instinct and appreciation of the actual world under the shell of much abstruse religious dogmatism. Yet, for a scientific development, these disharmonies between theory and practice must vanish, and the knowledge of facts must precede the shaping of the nomenclature.

The historical way of looking at the nature of things which has matured the theory of evolution, and which has modified traditional dogmatism generally, has also done remarkable service in psychiatry. After a period of purely ontological and narrowly descriptive work — Pinel, Esquirol and their pupils — Morel initiated the momentous study of the problem of degeneration, so creditably continued especially by French and Italian followers, and brought to perhaps an extreme climax by the school of Lombroso. Among the Anglo-Saxons the principle of evolution became the illuminating factor, in a more theoretical way it is true, to quite an extent, so that the facts were used as illustrations of the *a priori* plausible principle and so that difficulties in the explanation of the facts were made less painful by the free use of mere analogies in evolution and dissolution. The great merit of a Maudsley lies in this direction of the introduction of sounder and more plausible views; but works of this type have done remarkably little to stimulate to productive work. In the hands of Hughlings-Jackson, the greatest philosopher and investigator among Anglo-Saxon neurologists, the principle has been most highly and most fruitfully developed. His way of using the principles of evolution and dissolution as a setting for keen discrimination stands among the best products of Anglo-Saxon medical thought and work, and has been truly stimulating on the rare occasions when he touched on the

field of psychiatry, as in 'The Factors of Insanities' (*Med. Press and Circ.*, 1894, p. 615-619), where among other points he makes clear that so many symptoms are the function of normal parts without the guidance of highest level mechanisms. His work has lately been reviewed by Sir William Broadbent, in *Brain*, autumn, 1903.

A few facts from the official statistics of the State of New York are here presented as an instance of how the Anglo-Saxon psychiatric literature was echoed in practical life. They aimed at such a presentation of the facts as appeared compatible with the state of psychiatry since 1888.

Form of Disease.	Number.	Per Cent. of Total.	Number Recovered.	Per Cent. Recovered in Each Form.
Mania, acute delirious.	181	.25	32	18
Mania, acute.	11,239	15.5	4,849	43
Mania, recurrent.	1,559	2.2	499	32
Mania, chronic.	4,733	6.55	236	5
Melancholia, acute.	17,525	24	6,132	35
Melancholia, simple.	1,164	1.6	329	28
Melancholia, chronic.	5,935	8.2	333	5.6
Alternating (circular) insanity.	266	.35	18	6.8
Paranoia.	974	1.35	31	3.2
General paralysis.	4,799	6.64	1	...
Dementia, primary.	1,568	2.17	450	29
Dementia, terminal.	15,951	22.1	116	.72
Epilepsy with insanity.	2,861	4	53	1.85
Imbecility with maniacal attacks.	1,776	2.46	30	1.7
Idiocy.	189	.26
Not insane. ¹	571	.8
Unascertained.	937	1.3
Totals,	72,228		13,109	18.1 %

They are, unfortunately, not statistics of individuals, but of admissions from 1888-1902, including also mere transfers from one institution to another. The main types recognized are mania, melancholia, paranoia and dementia, general paralysis, epilepsy and imbecility. Mania means excitement, melancholia-depression, but only a certain number of cases are plainly the one or the other. The specifications are also little explicit. The groups mean little from the point of view of final outcome, as is seen from the percentages of recovery, which are, of course, not to be taken as final either, since many cases are transferred or discharged before they have reached recovery or the final stage. A comparison of the numbers from year to year and in the various hospitals shows obvious differences in the use of the terms, and the same holds for the various attacks in individual cases.

¹ Includes cases of alcoholism, drug habit, etc.

II.

Among the German writers, we see an interesting adaptation of the various stages of more or less dogmatic current systems of psychology, and a great development of experimental physiology and pathology. We find greater extremes, owing to the indomitable longing for logical consistency and the feeling of moral obligation to take a stand with one of the divergent schools of psychological thought of the period. There has, however, been a *growing confidence* in the paramount importance of more accurate observation of the actual facts of insanity, *the clinical evidence*, and its analysis along the lines of the psychological experiment and of the knowledge of aphasia and other topics of brain-pathology, and the result has been a wave of interest from which all the other countries are drawing fresh inspiration. The French had probably been the most patient guardians of a clinical method with which they laid the foundation for so many advances in general medicine and pathological anatomy; but apart from the necessarily somewhat dogmatic interest in hereditary conditions (Magnan) and in the somewhat limited field of neuroses (as in the noteworthy and classical studies of Ribot and Janet) and in the hazy auto-intoxication theory, there was an undeniable lull in the productivity of fruitful problems.

In the center of the modern movement of psychiatry we find before all others two German alienists, Kraepelin and Wernicke, who have given the most fruitful impetus to a revival in active interest in psychiatry. As an important factor we must further mention Ziehen, and also Sommer. These men have emancipated psychiatry from the peculiar position of an adjunct to neurology which was so prominent when technical advances attracted the attention to the virgin fields of spinal cord lesions and other nervous disorders. In those days it was possible that the chair of psychiatry was given to an anatomist, because he had published a very valuable study of the maturation of the tracts of the spinal cord and brain-stem; and that the main interest of the alienists in charge of the university clinics was directed largely to nervous diseases. Kraepelin has been faithful to psychiatry as such and to its immediate correlate, experimental psychology, without feeling a desire to adorn his position with the easily obtained laurels in neurohistology, and his faith has been rewarded amply. Wernicke, on the other hand, has grown into psychiatry from the neurological side, but also with high credit on account of his most scrupulous discriminations in symptomatology. Both these men had received a strong inspiration from Kahlbaum, and Wernicke also from his predecessor

Neumann, two writers who had been too painstaking for their times and unable to stem the tide of tradition and of microscopical and 'spinal' psychiatry, but who deserve to be mentioned among the classics of psychopathology even more prominently than Griesinger.

Kraepelin, under the influence of v. Gudden and Wundt, developed along two closely allied directions of work: *the clinical observation and the experiment*. At a time when others thought they could get at the nature of disease by arduous section-cutting and microscopy, Kraepelin realized that the great need was a careful sifting of the symptoms of the diseases according to their importance as determining the nature of the disorder; and that the real school of training for the clinical observation (continued observation such as can only be carried out in a well organized hospital or 'clinic') was the artificially produced abnormality, the experiment, in which we can determine accurately the make-up of the individual and his usual trend of reactivity and activity, and in which we can vary at will the etiological modifications, such as the quantity of poisons, or influences like fatigue, sleep, hunger, etc. From 1880 to 1895 this experimental work had, however, hardly brought out a very profound departure from current lines in psychiatry. A contrast liberated the strength of this position very emphatically. Ziehen, a former assistant, but not a follower of Kahlbaum, an extremely systematic worker, known by numerous contributions in the most heterogeneous fields, from anatomy, through experiments on epilepsy, to clinical studies on the pulse and on the gastric functions in the insane, and to a text-book of psychology—a writer who has, to-day, covered the whole field of anatomy, physiology and pathology of the brain, psychiatry, psychology, and even epistemology, and who has just been appointed to the most prominent chair of psychiatry in Germany—Ziehen was more or less responsible for bringing out the contrast which has given the school of Kraepelin a peculiar importance in psychiatry.

In Ziehen's *Textbook on Psychiatry*, 1894, he has made his psychology the foundation of psychopathology. His general psychopathology follows quite closely the chapters of his normal psychology, and matters which the physician is in a habit to see combined into symptom-complexes are here torn apart, for theoretical and systematic reasons. This would hardly have brought out the crisis, since the same can be said of Kraepelin's general psychopathology, which is also arranged in the form of a normal psychology—disorders of perceptions, of intellectual processes, of the emotional life and of will and action being dealt with successively, just as Ziehen deals with disorders of sensa-

tion, of concepts or memories, of the intellectual tones of feeling and affects, of association of ideas, and of activity. The chief distinction is the schematic associationism of Ziehen, and the freer adherence to Wundt on the part of Kraepelin. The actual clash occurred on the practical field, in the 'special psychopathology,' in which Ziehen's thorough-going consistency gave psychological distinctions an undue prominence at the expense of practical experience and a truly medical conception of disease.

Ziehen recognized affective psychoses (mania, melancholia, neurasthenia), intellectual psychoses (stupor, paranoia, obsessions), and compound psychoses (secondary hallucinatory paranoia, post-manic and post-melancholic stupor, post-neurasthenic hypochondriacal melancholy and paranoia, post-melancholy hypochondriacal paranoia, melancholic-manic insanity and catatonia). These affections 'without intellectual defect' he opposed to the defect psychoses — congenital (idiocy, imbecility, debility), and acquired (dementia paralytica, senilis, secundaria following focal disease of the brain, secundaria following functional psychoses, epileptica and alcoholica). Anyone familiar with actual groups of patients realizes that this scheme tears apart many well-established units, and puts side by side essentially heterogeneous entities, mainly to satisfy a psychological system—a fact which comes out even more strongly in the new edition of 1903.

Kraepelin in a review of Ziehen's book made front against this defect. He sees in the use of the association psychology an intentional neglect of important inner experiences which are forced regardlessly into a uniform pattern. The cortex is populated with concepts and nets of memories, beside which affects, character ('the sum of all the ethical tones of feeling'), and voluntary impulses have little space. He sees the greatest confusion in the chapter on paranoia. We find there side by side the chronic incurable states of delusions of persecution and grandeur, lasting the entire life, and the acute curable exhaustive puerperal psychoses, delirium tremens, periodic and circular disorders, and even the transitory deliria of epileptics. "We evidently stand here on the ground of hot and cold fevers, as they were spoken of before the days of stethoscope, thermometer and pure culture." Such a symptomatic grouping puts together similar temporary conditions, but not disease-forms. Everywhere, even in psychiatry, one should consider that the same causes have the same effects, and that wherever exceptions appear we must conclude that there is a difference in the factors at work. Kraepelin turns against the looseness of nosology resulting from Ziehen's emphasis on the

components of the symptom-complexes and the dogmatic tendency to explain entire diseases as combinations of elementary symptoms. Symptomatology of the type of associationist analysis cannot possibly be more than a part of psychiatry. More emphasis must be put on the other facts of nosology. To show this it would take a book, and he promises to write it.

Far be it from me to imply that Ziehen had neglected to make actual experience with the insane the foundation of his descriptions. The method, however, is one borrowed from psychology of a more didactic than practical character. Ziehen operated throughout with a constructive association-psychology, not with an essentially experimental attitude. A predetermined set of elements offers the material for permutations, while Kraepelin is somewhat more ready to give the facts of observation their rank according to their importance, more or less independent of traditional psychology, indeed with a very strong bias to more purely biological traits of reactions, the *form of the stream of events*, and a certain neglect of what is apt to preoccupy the philosophical (and logic) mind, *i. e.*, of the 'contents' of the psychic events.

III.

I have to ask the psychologist to be patient with the material which must be discussed here for a real understanding of the situation. It might seem that matters were involved here which have little to do with the field of interest of the pure psychologist. He is interested in the psychic manifestations, not in what a physician may want to stamp as a disease. But just those issues are the ones on which modern psychiatry turns and which must be understood if there shall be any common ground between psychologists and alienists. A great part of traditional *psychology* is unintelligible to the physician and biologist without some knowledge of the *philosophical* ground on which it grew. If he wishes to grasp it, he must make the sacrifice of certain collateral studies, which may appear abstruse and little gratifying, but are the only avenue by which to find out how it came that many an important 'law' of psychology has been created, for which he would hardly see a demand in his experience. The same demand meets him who wants to get acquainted with *psychiatry*; and we may say in favor of many recent efforts that psychiatry invites an increasing attention to actual events rather than to opinions. On the basis of empiricism, the student must get acquainted with the events in the life of the patients just as they happen, in order not to fall at once into mere imaginative constructions, or into the deplorable way of picking

out of its proper connections that which lends itself to startling illustrations of pet theories. Psychiatric facts, like all facts of natural science and especially of biology, should first be studied in their proper and natural setting.

Kraepelin, from his first edition, aimed to adopt as much as possible Wundt's standpoint in psychology, in his introductory part. In discussing the types of actual cases, he spoke of states of depression (simple melancholia and melancholia with delusions), dreamy states (hypnotism, somnambulism, lethargy, epileptic and hysterical dreamy states, stupor and ecstasis, and acute dementia), states of excitement (melancholia activa (!), mania, deliria, febrile and alcoholic), periodic psychoses (mania, melancholia and circular forms), primary paranoia, paralytic dementia, and states of mental defect, developmental idiocy, cretinism, anergic and erethic imbecility, sexual psychopathia, moral insanity, and querulant paranoia, neurasthenic states (*folie du doute*, mysophobia, agoraphobia, imperative impulses), senile dementia and states of secondary dementia (secondary paranoia and secondary dementia such as recovery with defect, agitated dementia and apathetic dementia). This plan shows a striking independence in the position of 'active (or agitated) melancholia' as an excitement and the position of senile dementia, but in the main a symptomatic attitude.

From a small volume of 384 pages the book grew to a large volume of 814 pages by 1895, and the seventh edition consists of two volumes of altogether 1,370 pages, and a totally different arrangement, at least in the 'special pathology,' which rubs most directly against the world as it is, whereas the plan of the general introductory psychopathology, the theoretical development, kept at least the general form with which it started.

In the special pathology, the third edition recognized deliria, states of acute exhaustion, mania, melancholia (simplex, activa and attonita), Wahnsinn (hallucinatory, depressive, expansive and catatonic), periodic and circular psychoses, paranoia (depressive and expansive), neurasthenic hysterical and epileptic psychoses, the chronic intoxications (seven forms of alcoholic psychoses, morphinism and cocaineism); paralytic dementia, acquired dementias (senile dementia, dementia with organic brain disease and secondary dementias), and anomalies of psychic development (idiocy, cretinism, imbecility and sexual perversion). We find here distinctly a tendency towards a systematic recognition of etiological groups, but also many symptomatic groups.

The fourth edition brings the beginnings of a fundamental change not in the general part (etiology and general psychopathology), but

in the recasting of types of disease. The effects of the teachings of Kahlbaum and Hecker come to the front, peculiarly enough in a form which is given them to this day by many opponents of the subsequent teaching of Kraepelin. The melancholia attonita remains as in the third edition: Wahnsinn forms a large group, but without a catatonic variety; Verrücktheit (paranoia) keeps its catatonic variety. But a new chapter is inserted as 'Processes of psychic degeneration': dementia præcox, katatonia and dementia paranoides. On the other hand, the paragraphs on secondary dementia are omitted.

The fifth edition is the book which Kraepelin promised to write as evidence in favor of his criticism of Ziehen. The 'special pathology' changed radically. We find the following disease groups:

I. The group of exhaustive and infective origin (post-febrile and collapse deliria; amentia).

II. The group of intoxication (alcohol, morphin, cocain).

III. The processes of disorder of metabolism (myxædema, dementia præcox, general paralysis).

IV. Insanity due to cerebral disease.

V. The disorders characteristic of the period of involution (melancholia, senile dementia).

VI. Constitutional disorders (manic-depressive insanity, paranoia, disorders accompanying the neuroses, idiocy).

Wahnsinn disappears completely, and a great process of simplification takes place.

The terms of a tradition of over 2,000 years are overthrown. The psychologist finds few of the old standbys left. The departures appear all to be away from psychological concepts. *Mental symptoms are dethroned unless they are characteristic of etiology, course and outcome.*

General paralysis, a hundred years ago a mere 'complication' of mental disease, now receives the position of a paradigm of mental disease as its most definite form. In the New York statistics, it is the one disease of definite outcome: it stands out as practically *uniformly* hopeless as to recovery. It not only implies invariably a progressive dementia, but also such organic disorders as will usually lead to death within a few years. Very probably it occurs only on ground of previous syphilitic infection. The mental symptoms are classed as fundamental as far as they demonstrate the characteristic deterioration; besides this, there are numerous temporary 'Zustandsbilder,' temporary pictures of a more accidental character, and relatively unessential for diagnosis and prognosis, although they are apt to impress the layman much more than the fundamental symptoms, and even to obscure the diagnosis by distracting the attention of the physician.

This disease is sized up as an *auto-intoxication* together with the rare thyreogenous insanity, and with *dementia præcox*, which is boldly extended so as to include all those cases of mental disease which lead to a peculiar deterioration usually termed secondary dementia, and *taken away from the concept of degeneracy* to that of a special 'disease process.' A more complete revolution of tradition has hardly ever been attempted.

Melancholia is again divided completely (see the first edition), but in a different manner. The agitated melancholia, which there was classed with mania, is recognized as a disease of the involution period, while simple melancholia, and recurrent melancholia and the non-deteriorating forms of excitement form one great unit, manic-depressive insanity. Degeneracy as such does not imply deterioration (because deficient endowment would only explain *arrest* of development, and any retrogression would have to be explained as a special disease-process; Vol. II., p. 270). In some etiological units, such as alcoholic insanity, there are, of course, many types of disorder and many types of outcome. Among the acute febrile and exhaustive disorders Kraepelin includes the cases with favorable outcome, and those leading to dementia join the group of cases with disorder of metabolism, dementia præcox. This latter disease may occur at any age, up to the fifties, and wherever a 'melancholia' or 'mania' passes into dementia, we should recognize that the disease was a disease process different from the start from those forms which never end in dementia.

These radical changes were mitigated somewhat in the later editions, but the main lines have remained fixed. On reading the successive editions one cannot help but deplore the fact that Kraepelin never published the clinical material on which his book is based, so that one might get at first hand an explanation of what led to the changes, and how the observations of the past appeared in the new light. Kraepelin uses a presentation in composite pictures which makes all control impossible. The contrast to Ziehen seems to me to explain part of the abruptness between the fourth and fifth edition, which tempts one to raise many a question.

IV.

My review limits me to the consideration of the psychological gain of Kraepelin's psychiatry. The alienist has to overcome two standpoints to become emancipated and an unbiased observer; that of the layman who follows the rules of mere plausibility and explains all

the actions of the patient in terms of possible motives of every-day life, with free admixtures of sympathy, or of annoyance, according to what the patient does, and that of the hardened dogmatic champion of tradition who forces the facts into a small list of antiquated names and fills in the gaps with vague patter of heredity, degeneracy, or anatomical theories.

Certain distinctions, which have become the first things one thinks of, lose their importance in view of a broader vista of events. *Depression* and *elation* are such contrasts; they determine the distinction between mania and melancholia, the chief units of our statistics. But depression is not necessarily characteristic of any special disorder. It may occur as part of a delirium, or of a delusional state, or of general paralysis, or of alcoholic insanity, etc. Unless we distinguish special types of depression we are not any wiser for speaking of it. To be sure several types are described under the heading 'Melancholia' of every text-book. But are they types which mean something definite in the entire working of the disorder? Kraepelin has the merit of having exploded the fascination of the general term and of having replaced it by more specific characterizations, which, in turn, are subordinated to *special types of course and outcome*. Depressions of the same etiology (or lack of definite etiology) and of the same type of course or setting are indeed found to present very characteristic traits of fundamental importance. The same holds for excitements (see Baldwin's *Dictionary*, articles 'Mania' and 'Melancholia'). Under manic-depressive insanity he describes the fundamental characteristics of those cases which present a number of attacks in a lifetime, but no essential deterioration, however severe the attack may be. Here, then, we see an instance of how Kraepelin's clinical attitude shapes a ground on which mental symptom-groups are subjected to *the consideration of events in the light of the outcome*, the very principle which makes experimentation what it is to us in scientific and ordinary life. Recurrent conditions which do not lead to deterioration are most probably events of a special type, and most likely a biological entity standing for a special kind of 'disease-process' or disease-principle. This one fact on which Kraepelin first insisted emphatically brings more definition into our knowledge than many a long analysis on principles of plausibility which are burdened with innumerable hazy considerations for heredity, severity of the attack and exaggerated claims of therapeutics. Many cases that are diagnosed as mania or melancholia are on careful investigation mere phases of this broad entity of manic-depressive insanity. The next

problem is to review the cases of recurrent insanity generally, to see to what extent the above picture needs extension to include all the non-deteriorating cases, running in attacks; further to study the cases with but one or two attacks and complete recovery; and the result has been the establishment of an extremely characteristic entity where before there was great confusion, as is shown in a case which appears perfectly plain and simple, as having attacks of this entity of manic-depressive insanity without deterioration, but which, in the eight successive attacks has given rise to the following diagnoses: Melancholia, mania, mania recurrent, melancholia chronic, mania chronic, dementia secondary to acute mania, mania acute, paranoia, chronic melancholia. This is an excellent illustration of the haziness of the older nomenclature and the lack of a larger view which would have given a correct grasp on the case even in the first attack: that the patient suffered of an essentially recurrent psychosis apt to appear in certain definite equivalents, with free intervals or more or less permanent recovery and without any tendency to deterioration.

Other depressions, of a fundamentally different character, are the depressions of the involution period, the alcoholic and epileptic depressions, and depressions of the deterioration processes. The latter, as we have seen, have been classed together by Kraepelin under the heading of dementia præcox, a much less definitely settled entity than manic-depressive insanity, also responding largely to the test of course and outcome, viz., the very frequent development of definite and unmistakable deterioration: emotional shallowness and apathy and intellectual dilapidation with often strikingly little memory defect, being at the bottom of the whole disorder. For a fuller statement of the characterizations of both manic-depressive insanity and dementia præcox, I must refer the reader to an abstract in Church and Peterson's *Nervous and Mental Diseases*, pp. 666-673. The perusal of the notes on dementia præcox shows that Kraepelin has boldly wiped out the old concept of secondary dementia; that he has made use of the undeniable experience that most cases of secondary dementia are not so invariably characterized by heredity, early constitutional defects in their start, or the 'severity' of an attack of 'melancholia' or 'mania,' as they are stamped by a peculiar cast of symptoms of a special form of deterioration. Many cases are perfectly characteristic from the onset of the disorder; and Kraepelin makes it plausible that this holds for practically all the cases which take the final course to 'secondary' dementia of the type defined above. This again shapes a definite problem for psychological investigation of a special type of developments.

In this group, which includes 14-15 per cent. of the admissions to Kraepelin's clinic, the dilapidation of thought and judgment, the emotional indifference, the development of old mannerisms, stereotypies, negativism and catalepsy, and a number of physical symptoms are referred to. As in manic-depressive insanity, we are relieved of the absurdity of changing the name of the disease successively from melancholia, to mania, confusion, paranoia, and finally dementia in various steps of the disease, where it is possible to form an estimate of the entire type of events at the outset, and where we can prove that it is a melancholia, mania, etc., different from types belonging to another entity.

We have not left the symptomatic ground, but instead of putting the weight on formal divisions of types, we see here efforts to get at the broad equation of the working behind the depressions, etc. That which looks alike but furnishes different results, is a topic of renewed investigation and expected to show factors formerly overlooked.

Kraepelin's great merit is to have reduced a number of dogmatic general considerations to their actual *value in the stream of events in the life of definite patients*. With an extremely sound instinct, he started from the field of experimental investigations and furnished excellent material for the type of psychology which medicine needs, a psychology which may be a hybrid of physiology, general biology and a certain utilization of introspection, but aims at the determination of the value or bearing of definite reactions and events in the stream of biological regulations. In this respect psychiatry, like comparative psychology, must learn to submerge the introspective method to the position of a mere help for the greater end; that of doing justice to mentation in terms of biological regulations, and of studying the possibilities of modifying these regulations. The 'elements of the contents' as they often are called, the usual elements of psychology, and hallucinations and delusions, count merely for what they are worth in the stream of events, be they of medical, or educational, or other practical bearing.

The chief directions in which Kraepelin has stimulated his associates to work psychologically, are a number of more biological than purely psychological reaction-types, such as retardation and inhibition, flight of ideas, etc. In this frame, he has utilized his methods of psychological analysis of functional efficiency (see the review of his work and that of his followers, by Dr. Hoch). In his own psychological work he has very wisely limited himself to the fundamental workable topics, and in his book his experimental work is given a

very modest position in the midst of the more roughly empirical issues. A few of his followers have been more strongly bent on more hazy issues, such as the explanation of catatonic symptoms or the fundamental disorders of deterioration (Stransky, Vogt, etc., in the *Centralbl. f. Nervenheilkunde*) with some success. But they have not dispelled the feeling that the fundamental traits of Kraepelin's 'disease-processes' are much more descriptive evidence of the *fait accompli* than illustrations of the working of the disorder in that form which above all satisfies our mind, in terms of cause and effect, and that it discourages such attempts as rather hopeless—which no doubt most of them are at present. A systematic trying and trying over again of what causal chains one has ground to suspect, would, however, be better than a dogmatic rebuff. There is something in the whole plan which raises the satisfaction with a dogmatic 'diagnosis' above the desire of a causal understanding. To allow practical advantages, a promise of a definite prognosis of course and outcome, to play such a fundamental rôle in nosological and pathological hypotheses implies a danger for sober consideration.

Yet he has brought home the importance of large frames for the details of analysis. He has shown the undesirability, if not futility, of experimentation with psychological methods which do not serve in the elucidation of some broader process. This is not the place to enter upon a criticism of the indirectness of much of Kraepelin's argumentation (see, for instance, his vindication of 'melancholia' in his last edition, Vol. II., p. 460), of the scanty differentiation of many points which are called fundamentally different, and of the question why general paralysis, and not some less uniformly working disease-process, should be the paradigm of psychiatry.

V.

A totally different impetus for psychiatric analysis comes from Wernicke. Kraepelin formulates his pictures in something like the terms of an experiment—certainly the most stimulating frame of human knowledge. He keeps the determining factors and the result in evidence, and for want of accurate and workable facts, he leaves the *detail* of the working as a matter to be taken for granted. Kraepelin evidently does not aim to settle the question how and why delusions and hallucinations and the various symptom-complexes arise. They are matters given by experience, and what concerns us is: under what conditions do disorders arise, and what do the manifestations tell us concerning the probable outcome and the means of modifying

the stream of events favorably. Our ignorance of the working of the details is a matter of regret, an inconvenience. It will be taken up when its time comes, as chemistry was taken up when physics was well advanced. Wernicke remains much more closely in a field of construction of a more geometrical world, not so much a world in a steady motion and development of experiments, but a study of the machinery itself. He aims to learn how to explain the symptoms of insanity out of the disorders of function of definite organs, in an essentially materialistic sense. Kraepelin certainly uses what facts there are, but his instinct of work makes him avoid a purely hypothetical field, such as the explanation of nervous and mental functions out of the fragments of anatomical knowledge will continue to be for a long time to come.

In Meynert's foot-steps, Wernicke starts from his own investigations on aphasia and he is strongly inclined to present all mental disorders in terms taken from the theory of aphasia. This anatomical bent is, however, excellently balanced by a strong sense for clinical observation, a keen eye for events in the patient's life and a remarkable freedom from the psychiatric common-places. Perfectly unbiased, he looks for his entities of description, absolutely unconcerned about tradition with its uncontrollable use of heredity and vague etiological factors, and of schemes of psychology and of nomenclature. Wernicke comes, indeed, nearest creating a psychology for his own needs.

For a sketch of his work, I must refer the reader to Church and Peterson, pp. 652-660 and 676-686. I shall attempt to review the most vital points here.

All mental disorders are to him disorders of identification (in some such sense as deviations from doing or thinking or feeling the right or adequate thing in presence of a special fact to be 'identified,' or, as I should prefer to say, reacted to properly). He speaks of disorders chiefly in the sensory sphere (sensory identification) or in the intrapsychic sphere (intrapsychic identification) or essentially motor disorders (of psychomotor identification.)

Primary identification, the simple perception of a sound as sound, is distinguished from secondary identification, which demands the co-operation of the mechanisms of the concept. The stimulation of the latter leads to intrapsychic adjustments (from the simple concept to a purposive concept), and these find their equilibration in the reaction of the psychomotor elements. In the psychosensory path or sphere we may experience anæsthesia, hyperæsthesia, or paræsthesia, in the

intrapsychic sphere afunction, hyperfunction and parafunction, and in the psychomotor sphere, akinesis, hyperkinesis and parakinesis. Under the spell of Meynert's teachings of cells and association paths, Wernicke does not resist the temptation of identifying special concepts or 'memories' with definite cells, without much scruple about the fact that the term cell has no sufficient scientific definition in functional neurology to-day. Wernicke's main point in the acquisition of memories is that the cells which are directly connected with the apparatus of projection (*i. e.*, the path from sense-organ to cortex, or from cortex to muscle or gland), are connected with one another by association fibers. The constant or frequent simultaneous excitation of definite sets leads to the formation of concepts, and (*Grundriss der Psychiatrie*, p. 35) these psychological units might be represented by some anatomical units, cells of definite strata of the cortex. He favors the view that the cell-layer nearest to the white matter, the first layer to be reached by impressions, is the representative of somatic consciousness, the impressions from the body (p. 47). The next step is the coördination of sensations of motion and position, innervation and memory, touch-sensation and touch-concept out of these organic responses, and a final step the elaboration of the consciousness of the personality, presented as a function of the consciousness of the external world and of the body. The organ of consciousness is thus populated with a sum of potential energies, memory-pictures and complex memories. These *contents* of consciousness belong to three natural fields: the external world, the body and the personality. This machine of numerous potentialities is next studied from the point of view of *activity*, the preformed order of associations, the concentration of consciousness, attention and will, the capacity of registration (*Merkfähigkeit*), the affects and the normal and abnormal values of concepts; but, here again, it is not the broad sweep of events, but the detail that absorbs Wernicke's attention.

This attempt at a medical psychology gets most of its material from the needs of clinical analysis of symptoms. It is full of hypothetical constructions, but on the whole, on the ground of well-founded analogies. It is a consistent elaboration on neurological hypotheses.

His views have been discussed before in the *PSYCHOLOGICAL REVIEW* (Vol III., p. 512) and especially by Professor James (Vol. IV., p. 225). I shall attempt here to outline Wernicke's position in the actual working of psychiatry and the recognition of types of psychic reactions.

Wernicke begins his descriptions of patients with types which show very little disturbance of the form of mental activity, but essentially disorders of the contents. He gives his theory of sejunction as the common link of many psychopathological states and of what we know of brain pathology: the severing of association, either in analogy with the focal lesions which usually underlie aphasia, or in analogy with degenerative neuritis in which the degeneration takes place with a selection of functionally differentiated associative elements.

Under this group of disorders, essentially of the 'contents,' he describes the paranoic conditions, and he opposes to them the acute psychoses in which the disorder of the *activity* of consciousness is uppermost. Throughout he chooses the very excellent plan of starting from one or more actual cases, which are well described and then analyzed. This gives the whole work a much more direct character than Kraepelin's composite pictures. He feels free to pick out the essential traits of disorder according to essentially medical needs. His descriptions have no equal in the entire literature of psychiatry. Everywhere we see a presentation of a palpable observation, and only that which is expressed by a patient and actually established is used for analysis. There is no resorting, either, to assumed anæmias or hereditary predisposition or to degeneracy. And perhaps even too much care is taken to avoid the grounds of etiology, course and outcome, fully as safe as that of hypothetical psychological constructions, however carefully held down to facts.

The actual needs in the analysis of special cases lead Wernicke to emphasize a number of points which are valuable additions to psychopathological terminology and concepts. He speaks of dominant ideas, imperative ideas, 'autochthonous ideas,' and phonemata, as a series of disorders of correlations of memories and 'Merksysteme,' under the principle of sejunction. He ascribes them to the sphere of the patient's personality, his body and the outside world, as autopsychic, somatopsychic, or allopsychic. He does full justice not only to the sensory components of difficulties, but also to the motor side (in pseudo-spontaneous motion, motility psychoses, etc.). He is apt to group together cases that have in common a special prominent symptom (the group of *anxiety* and *somatopsychic disorders*), then the disorders essentially of the *autopsychic sphere*, including the types beautifully described as simple mania, and as affective melancholia, identical with what Kraepelin would include in manic-depressive insanity. Further, he points to the *allopsychoses*, such as alcoholic deliria, acute hallucinosis, etc., and to the *motility psychoses*, in which the

fundamental disorder would seem to be one of the higher muscular coördinations.

The difference of standpoint comes out most strongly in connection with general paralysis, which he admits as an etiological complex, but in which he takes pains to point carefully to the great *variety* of mental disorders on the ground of the fundamental deterioration. He carefully analyzes types which Kraepelin is inclined to submerge, and looks forward hopefully to a utilization of such analyses in efforts to determine more carefully the localization of disorders which express themselves in special symptom-complexes.

He distinguishes the following mental disorders in general paralysis:

1. The expansive autopsychosis (the megalomania of older writers).

2. Paralytic mania (not always distinguishable from pure mania, and frequently passing into the expansive autopsychosis, or at times into a remission).

3. Affective melancholia (depression with feeling of insufficiency), rarely pure, usually with an admixture of delusions of reference or with symptoms of an anxious psychosis.

4. Depressive melancholia (akinetic or hypokinetic depression), also often with admixtures of delusions of reference.

5. Among paralytic allopsychoses we meet frequently a paralytic delirium, which may occur at any phase of the disease, and at times resembles closely a delirium tremens, but more frequently a stupor with disturbed sensorium; or

6. The presbyophrenic complex occurs, or a chronic hallucinosis, or dream-states, resembling epilepsy, although but rarely, whereas the anxiety psychosis is not infrequently of paralytic origin; also hyperkinetic and akinetic motility psychoses (akin to catatonia) are represented.

The results of Wernicke's work coincide in many points with results of Kraepelin's totally different method. We may well say that the two writers supplement each other, as I have tried to show in my review in Church and Peterson.

To be sure, Wernicke's subdivisions are to a large extent pure empiricism with a rather artificial and not sufficiently founded brain-pathology and psychopathology as a leading thread in the labyrinth of facts. He unnecessarily slights the issues of outcome of which Kraepelin makes so much. But no one can read his book or even the abstract of his work without coming across a wealth of well-chosen

points which would have remained in the unclassified residuum but for the bold breaking away from tradition, a strict adherence to what is at hand in the patient and as good a utilization of brain pathology and natural subdivisions as is available to-day. Wernicke's honesty in admitting that he cannot do justice to more than one half of the cases he meets, is another grand step away from the hit or miss classifications which governed psychiatry so far.

Concerning strictly psychological gains, Wernicke will probably exert a rather strong influence through his pupils Liepmann and Storch.

Storch is developing a deepened psychological utilization of the neurological data which shall form the subject of a later review.

VI.

A final contrast may bring out the present situation of psychiatric investigation. Professor Ziehen¹ points to the futility of expecting an absolute classification of the facts of psychiatry. Whatever division is used ranks according to the extent to which it does justice to the two issues of an intelligible terminology and its adaptation to the progress of investigation. He, to my mind very justly, questions the feasibility of a uniform classification from only one point of view. Groups may be formed according to the course of the entire symptom-complex, according to etiology, pathological-anatomical data, etc. Each may have its advantages without excluding the other. For educational purposes it is decidedly advantageous to view the facts from more than one point of view. For instance, to review on one day all the psychoses which originate on the ground of chronic alcoholism; and another day all the etiological factors of a certain psychosis of usually alcoholic origin, such as acute hallucinatory paranoia (hallucinosi or amentia). We thus may come across monopsychic and polypsychic etiological factors (*i. e.*, etiologies producing but one disease-type, and etiologies entering as a component into many different pictures), and monoetiological or monogenous and polyetiological or polygenous psychoses.

Far more important than the *uniformity* of divisions is the *completeness* of our method of grouping, with due consideration of the *types* and of the *transition forms*. Not to recognize the latter has led to many errors. Such 'convergences' of two psychoses are illustrated by the so-called delirium acutum (to my mind not the best instance, inasmuch as acute delirium is merely a peracute form of

¹ 'Ueber einige Lücken und Schwierigkeiten der Gruppierung der Geisteskrankheiten,' *Monat. f. Psychiatrie und Neurologie*, Vol. XV., No. 2, 1904, p. 147.

almost any mental disorder, without actually being a transition form leaving the original ground). He strongly favors more careful consideration of individual characteristics referable to individual peculiarities. Ziehen promises to analyze his great collection of 700 puberty psychoses from these points of view.

Nissl,¹ a follower of Kraepelin, takes this programme for a starting-point of a scathing criticism, which shows what an extreme and exclusive elaboration of a monoideistic scheme, however good it may be, would lead to. Ziehen's reasonable conclusion that more than one point of view is needed to do justice to psychiatry, forces Nissl to the conclusion that psychiatry could not be a medical science, because the possibility of grouping the facts from *one* point of view is a 'self-evident' requirement of medical science. He claims that we are practically ignorant of the real cause of mental disease; that our methods for the study of the symptom-complexes are altogether too rough to properly differentiate the criteria of similar symptom-complexes in various disease-forms. In pathological anatomy we are about ready to begin with the beginning; hence, there is only one chance, that is to group the psychoses according to — the clinical picture. To us that would mean the sum of our evidence, including etiology, course and outcome, and, in case of death, what a careful anatomical study reveals. But Nissl² anchors on pathological anatomical hypotheses, with the following dogmatic climax: "In almost all the functional psychoses it is possible to demonstrate anatomical findings in the cortex. As soon as we agree to see in all mental derangements the clinical expression of definite disease-processes of the cortex, we remove the obstacle which to-day makes impossible all agreement among alienists." Yet, when we look for facts, it becomes plain enough that anatomy has so far furnished too few decisive facts, and that those hypotheses are the best which are based on evident and controllable facts in a field easily brought to a test of experience or experiment; and such experiments are more likely to be fruitful along functional lines than along simple anatomy, in most of the diseases with which we deal. What does an anatomical dogma help?

Ganser's description of random replies in a peculiar mental state of prisoners had given rise to a controversy as to their 'hysterical' or 'catatonic' nature. Owing to the difficulty of defining absolutely

¹ Kritische Bemerkungen zu Ziehen's Aufsatz: 'Ueber einige Lücken und Schwierigkeiten der Gruppierung der Geisteskrankheiten.' *Cent. f. Nervenheilkunde u. Psychiatrie*, No. 170, March, 1904, p. 171.

² 'Hysterische Symptome bei einfachen Seelenstörungen,' *Centralblatt f. Nervenheilkunde u. Psychiatrie*, Neue Folge, 13, 1902, p. 2.

what distinctions are at issue, Nissl takes a dogmatic anatomical standpoint. Kraepelin, for excellent practical reasons, urges that one should only speak of hysteria where there is 'a chronic constitutional condition with paroxysms presenting throughout a condition of increased influence to emotional reactions on the body.' Such a statement is quite justifiable as long as one keeps in mind that it leaves the 'formes frustes' and episodic forms unaccounted for. In some of these 'formes frustes' it is obviously best to submerge the *hysteriform* character of the symptom-complex in favor of a broader concept of disease-forms of a definite course and outcome, which may be different from true hysteria, and are therefore to be kept apart for practical and possibly even strictly pathological reasons. In order to make that which is perfectly true and most helpful in his propositions bear the air of dogmatic assurances, Nissl brings in his anatomical prestige, and claims, 'we understand by hysteria a congenital disease which brings with it a peculiar condition of the nervous system, which shows clinically by the development of the so-called hysterical character and is lastingly active inasmuch as at any time transitory physical disorders and various forms of a specific insanity may be produced by strongly emotive ideas.'

Ganser saw in random replies a hysterical symptom; Raecke a hysterical stigma; but since some of the cases showed evidences of some deterioration, Nissl claims the symptom as one of 'catatonic negativism,' because no case of true hysteria deteriorates. According to the law of the outcome, deterioration processes are most frequently dementia præcox (including catatonia). The analgesia, abasia and aphonia in these cases is nothing but catatonic negativism (p. 20), and the fiasco of the symptomatological method is obvious (p. 24). Its advocates or habitués are naturally not conscious of their folly; their very method, the symptomatological method, has a serious defect of character (p. 26); there is, according to Nissl, no objective measure for the test of its hypotheses; hence it lacks the possibility of a correction of the error. This feature does not seem to him sufficiently appreciated. The 'clinical' method dispenses with all psychological explanation and hypotheses. It uses altogether the general principles of natural science investigation. He enters on a peculiar crusade against the supposed folly of building any conclusions on the psychic activity of a diseased brain, an attitude which would naturally appeal to those who do not see yet how psychology *can* be a field for pathology.

In a very recent article Ganser assures us that he is familiar with Kraepelin's katatonia concept, and that he nevertheless stands by the

hysterical nature of the majority of his cases of random reply. As things stand, we obviously are forced to admit that the symptom of random answer is not necessarily negativism as Nissl claims, nor necessarily a hysterical stigma as Raecke says, but a symptom of relative value only, like most psychiatric symptoms.

This whole discussion may be of little interest to the psychologist, but it illustrates vividly the great difficulty of the alienist. He is confronted with so many relatively uncorrelated and heterogeneous trends of experience; yet, most of the training given us in our education is built on the theory that one or two series of facts must suffice, and all the heterogeneous experiences must be forced into a statement of a single or double series of data. This is, for the present at least, an utterly untenable working hypothesis. Unless one has a chance to use with ease, and with a feeling of justification, a free pluralistic method of dealing with things, dogmatic restrictions kill off many a possibility of seeing things for what they are worth. The greatest need we alienists have is a general recognition of sound pluralistic principles of experimentation, and in this direction the Anglo-Saxon mind with its empiricism seems to have a strong interest. The logic of a sound empiricism has no set form of classification, and is free to recognize the unfinished character of many of our stipulations. The value of things is determined by their working value rather than by the logical harmony of the picture, within certain limits at least. The emphasis of one point or another appears as the personal contribution of the temperament of individual observers. And while every one is given full chance to carry into the field the best array of facts, nobody is expected to make a finally exhaustive and still less an exclusive system.

Gaupp, a former pupil of Wernicke, now a pupil of Kraepelin, although strongly under Kraepelin's influence, has published two noteworthy papers¹ which show a much more conciliatory attitude. Everything tends towards a less exclusive appreciation of anatomical facts; etiology, development of symptoms and outcome are recognized as points to be considered in every case, but only for what they are worth, and yield in the way of furnishing facts for a presentation of any disorder in terms of cause and effect, in a measure as they point to means with which the course of events may be turned into favorable currents (therapeutics), and to principles of a more correct understanding of the actual events (pathology).

¹ 'Ueber die Grenzen psychiatrischer Erkenntnis,' *Centralblatt f. Nervenhilfkunde u. Psychiatrie*, Neue Folge, XIV., January, 1903, p. 1. 'Zur Frage der combinirten Psychosen,' *ibid.*, p. 766.

In this large frame psychology is bound to play an important part, but it must be a psychology in line with the broad psychiatric postulates. The demand for an absolute explanation of all abnormal mental activity is modified and given more workable shape in the study of the nature of determining conditions and the nature of the value or meaning of a definite mental state for the further development of the life of the patient. The charm of hypothetical correlations of definite brain states with definite abnormalities of mental reaction becomes reduced to its heuristic value in the study of nature's experiment; and the study of nature's experiment is best carried out with helps of the character of an experiment.

Breaking away from the soporific influence of tradition, Kraepelin has pointed to the relations of broad clinical pictures to fundamental symptoms, and he has given us very valuable perspectives for the formation of a broad frame of events for detail studies. Wernicke has kept alive certain interests in localizatory correlation (sejunction theory, etc.), and pushed his symptomatic differentiations in the direction of picking out the central events of several symptom-complexes (anxiety psychoses, motility psychoses, etc.). Janet, who has worked on a field much more accessible to the psychology of hypnosis, has given definition to an important group of neuroses, and to certain possibilities of synthetic reconstruction. There is, of course, everywhere a tendency to exaggerate a principle of work into undue proportions, and thus to stifle in a way the cropping out of new interests. But with proper organization of places for careful observation and study of patients, and with the necessary helps from the contributing biological and general pathological sciences, a more planful and at the same time more specializing spirit of experimentation can arise without too great danger of fragmentation.

Psychology contributes essentially in the problem of symptomatic differentiation; and to some extent as the cardinal force of therapeutics. The study of mental disorders in gross brain lesions on the one hand, the study of the bearing of definite physiological, toxic or pathological states of the mechanisms of the physical household, and the study of the weight of special mental attitudes and habits in the life of persons of various make-up assist in shaping a special medical psychology.

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A REVIEW OF SOME PSYCHOLOGICAL AND PHYSIOLOGICAL EXPERIMENTS DONE IN CONNECTION WITH THE STUDY OF MENTAL DISEASES.

The new impulse which is everywhere felt in the study of psychiatry depends largely upon the fact that the necessity of careful clinical observation has become more thoroughly appreciated. We have passed through a period in which the chief salvation was sought in the study of anatomy and pathological anatomy of the nervous system, and in which clinical studies were more or less neglected. This tendency has now been overcome, while the anatomical studies are by no means lost sight of.

In the clinical studies which were undertaken the necessity of a more accurate analysis soon made itself felt and this led to the development of careful tests and of the adaptation of the psychological and physiological experimental methods to the special problems of psychiatry. Especially Kraepelin has been the pioneer in this field.

Experiment will not replace clinical observation, and it would be short-sighted to suppose that all that may be found out by a mere study of the cases without the aid of tests has already been discovered. But experiment will help to a finer analysis of symptoms, and clinical observation and experiments will mutually aid each other, each furnishing problems for the other, and each assisting the other in the further elaboration of the results, for after all the methods do not differ essentially. Aside from the study of mental disorders as we meet them in our patients, experimentation has been wisely used for the study of slight 'mental disorders' artificially produced by drugs, exhaustion, fasting and the like. These latter studies have not only aided materially in the development of methods but have also given us causal chains, and have demonstrated how *different causes affect our mental mechanism in different directions*. They have in the hands of the investigators, especially of Kraepelin, furnished us with excellent analyses, which have already become fruitful for the study of mental diseases, and which will certainly prove of still greater value in the future. We must clearly appreciate that all these studies are as yet in their infancy, and that no startling results can be expected so soon, yet no one familiar with the work can doubt that the road thus far covered is the right one.

We shall take up the different studies under different headings, so far as this is feasible, at the expense of having to mention the same work in different places, and we shall, for want of space, treat the studies on mental diseases alone somewhat more fully, while those on artificially produced conditions have to be dealt with more briefly, though the analyses put down in these papers deserve much consideration and study. The process of *apprehension* (Auffassung) was studied by Kraepelin and his pupils. Cron and Kraepelin have developed a method suggested by that of Cattell. On revolving drums were pasted, spiral fashion, monosyllabic or bisyllabic words or senseless syllables, so that, when the kymographion was set to work, these words appeared successively behind a slot. The time of exposure could be varied. That time was chosen which did not allow a clear apprehension of all words. It was possible in this way to study apprehension by means of a continuous method. The number of units recognized could be used for a measure of the excellency of apprehension, while the quality could also be studied. Another method was that described by Finzi. Before a card containing either figures or letters in various groupings a diaphragm is allowed to pass quickly by means of a spring arrangement. The test most extensively used was that of nine figures arranged in three rows of three figures each. The experiments made with these methods refer chiefly to drugs and artificially produced states, and only to a small extent to the study of patients. Among the former there were studied especially alcohol, paraldehyde, trional, bromide of sodium, caffeine, bodily and mental work, and fasting. It was found that the hypnotics par excellence, namely trional, paraldehyde and alcohol, markedly influenced the process of apprehension, and it was suggested that it is upon this essentially that the hypnotic action of these drugs depends. This is aided in the case of trional by a quieting effect which was demonstrated for this drug upon the motor processes. It is interesting that fatigue has the same depressing effect upon apprehension as was shown by the study of the condition produced by bodily and mental work. In the latter the quieting effect upon the motor processes is also present. Fasting, however, did not influence apprehension, though it showed marked effects in other directions. Bromide of sodium produced no effect in the experiments of mono- and bisyllabic words, but some improvement was seen in the experiments with senseless syllables under the influence of this drug. This curiously isolated action was of course not attributed to a beneficial influence of the drug upon apprehension but was explained in a different manner. Loewald

has shown that bromide relieved certain feelings of discomfort, and as the tests with senseless syllables, unlike those with words, were associated with a distinct discomfort, Ach, who made these tests, attributes his results to this action of the drug. Bromide therefore has no action upon apprehension as such. Caffeine had a beneficial effect upon apprehension, both omissions and mistakes diminishing under its influence, and a measurement of apprehension time which was undertaken in a few experiments, also showed improvement. Reis made studies with these methods in cases of general paralysis and dementia præcox. The result was especially striking in general paralysis, in which the threshold of apprehension was very low. In cases of dementia præcox the changes were slight and were moreover influenced by the lack of interest which these patients showed.

The reduction in mental productivity found in various forms of dementia and the changes in coherence of thought noted in different states made it probable that by systematic studies with so-called *association experiments* some valuable help might be obtained for an analysis of certain mental disorders.

The most extensive investigation we owe to Aschaffenburg. He has elaborated tables for the different categories of associations, based essentially upon Wundt's teachings. A. then studied the effects of exhaustion and of the manic states upon the nature of associations. The reason why these two states were studied is because we find in mania as well as in the so-called exhaustion psychosis what is called flight of ideas. This may briefly be characterized by saying that patients who present this symptom do not stick to their subject but run from topic to topic, their train of thought being continually deflected by external or internal happenings. In more pronounced degrees the tendency to rhyming becomes marked and the more the condition progresses the greater is the lack of internal relation. In his investigation A. used two methods, (1) associations were demanded to individual nouns; sometimes the association time was measured, (2) a continuous method in which only the first word was given, while the associated word served as stimulus for the next, and so on. A. found that exhaustion and mania lead to a diminution of associations according to content and to a marked prevalence of sound associations. He also found in contradistinction to what has been claimed by others that the association time is never shortened; on the contrary it may be lengthened. The prevalence of sound associations was also found as a result of alcohol intoxication, and in experiments with fasting. In all these conditions there were also signs of greater

motor responsiveness. For this reason, and since A. regards with Kraepelin the word, irrespective of its content, as essentially a motor image, and defines sound associations as associations united by a motor tie, he seems to assume that these motor images share in the general increase in responsiveness of all voluntary motor processes, and that consequently associations within their sphere (similarity of speech movements) are favored. At any rate A. regards the appearance of sound associations as a direct result of the motor excitement. Again, if he asked a normal subject to write down without choice all ideas which arose one after the other, he found a sequence of ideas not unlike that seen in mild manic patients. This was regarded as a strong evidence that the transformation of thought into speech without choice, a condition which seemed to A. to exist in loquaciousness (as a part of the general motor excitement), was of great importance for the development of flight of ideas. From all this A. concludes that the flight of ideas is secondary to the motor excitement. This view certainly does not agree with clinical experience, and it has been attacked from various sides, most successfully by Liepmann, who justly lays the chief stress in flight of ideas upon a disorder of attention. But even the deeper relation between sound associations and motor excitement, such as A. suggests, seems to me questionable. In the first place we have no right, as Liepmann points out, to identify the word with the motor image only. Still more important is the fact that if we look through all the experiments made in Kraepelin's laboratory in which both the nature of associations and the motor processes were studied, we are struck with certain inconsistencies which must make us cautious. Indeed it seems less forced to assume that in all the artificially produced conditions in which sound associations were found, these are essentially due to an attention disorder. This is all the more plausible since we find the simple mental processes such as continuous calculation regularly affected in the same conditions. Aschaffenburg himself felt the necessity of excluding other factors in the production of sound associations and showed that at any rate a disorder of apprehension cannot be made responsible, since such a disorder is especially marked after trional though there is no change in the nature of associations. This is true, but it is very probable that a disorder of apprehension may exist without alteration of attention.¹

Sommer and his pupils insist in their association experiments upon the value of having a fixed set of stimuli, and Sommer therefore

¹ It would lead us too far to discuss this whole question here in detail. I intend, however, to do this more fully elsewhere in a review of some recent papers on flight of ideas.

has given us a definite list of words which may be used in such a study. This has the value of furnishing us with comparable material. The objection that with the repetition of the same tests certain associations become fixed is undoubtedly valid, but this very factor may be worth studying, as Wreschner pointed out, while on the other hand the error may be avoided by a sufficient separation of experiments. In their studies Sommer and his pupils place less value upon the special categories of associations, which after all are problematical, but they lay their chief stress upon certain deviations which clinical and experimental experience had taught them to be important, such as a productivity and range of ideas, or their stereotypy, the intellectual value of reactions, sound associations, and certain strange unaccountable reactions (*dementia præcox*). Such a tendency arose from Sommer's practical aim, *i. e.*, the elaboration of differential diagnosis. In his book he gives us instructive examples of cases studied by this method. Both he and Fuhrmann investigated epileptic dementia. They found a more or less marked diminution in the extent of production, a certain monotony in the mode of reaction, and a marked egocentric element in the reactions, sometimes with a religious coloring. The egocentric element was in one of F.'s cases so marked that reactions occurred which bore no relation to the word given. These experiments agree with clinical experience, and certainly show how, by means of such tests, a better insight may be obtained into the characteristics of dementia. Bleuler has, with methods of his own, studied the deterioration states of *dementia præcox*, and has shown that unaccountable reactions are especially frequent here even in more or less mild cases. This result may be of considerable importance both diagnostically and for a clearer understanding of the disease as such, of which very probably the element of dissociation, if we may use this term, is characteristic in the active as well as the demented stage. We see, therefore, that with these association experiments we have a means to a more accurate study of dementia, while they have also given us certain analogies between the manic and some artificially produced conditions which are qualified to lead to a better understanding of mania.

Reaction time and simple mental processes were studied in conditions produced by different drugs, as well as in various mental and nervous states. For want of space we shall here consider only the latter. But we should first mention in this connection that Kraepelin has instigated a large number of experiments in which the influences were studied which various factors, such as rest or fatigue, practice, warm-

ing up, etc., have upon the *course* of the work. He has united all these results in his masterly paper upon the 'work curve.' Such studies were of course necessary before any alteration in the work curve could be interpreted.

Gross examined a patient who met with a railroad accident without receiving any marked bodily injury. His mental symptoms were essentially characterized by forgetfulness, drowsiness and weariness. The patient was first examined on three successive days, five months later on four successive days. G. used the following methods: (1) adding single figures (printed in vertical rows for such studies). At a clock signal a mark was made every five minutes. Hence the course of the work could be followed up. It was found that, compared with 21 normals, the patient stood lower than anyone else. The normal persons who were nearest his level showed an uncommonly marked rise in the second, compared with the first 15 minutes, while the patient presented a progressive, marked fall, hence an abnormal tendency to fatigue. But on the other hand an analysis of the different experiments showed that the practice curve was not abnormal. (2) Counting from 1 to 20 five times in succession also showed evidences of increased proneness to fatigue, and was done more slowly than normally. As the writing balance showed, however, no abnormality, this slowness could not be due to a motor retardation, but was attributed to articulatory disturbances which are not infrequent in these cases. The process of apprehension was also weakened. For practical purposes these results are important because their regularity excludes the possibility of simulation and it is this which is so important to establish, since many of these traumatic neuroses and psychoses are litigation cases, and since we have very few objective tests for these conditions.

It has been found that epileptics may have not only attacks of marked mental disorder but also periods of slight deviations which still await a clear analysis. Gross examined the reaction times in three such cases. In two of them he found a decided increase in the reaction time for simple reactions, while vowel reactions, and especially choice reactions, were less affected. He attributes this to the fact that the empty impressions, such as a mere sound, which have no associative support, need a special strain of attention to be clearly apperceived. In the third case he found at first a lengthening, later a shortening, especially of the choice reactions. G. explains this on the ground that at first there was a greater difficulty in the liberation of motor impulses, later a facilitation. This agreed with the clinical analysis.

Ranschburg studied the quantitative and qualitative differences in reactions between the young and old (subjects without senile dementia). He found simple reactions, choice reactions, word-reading reactions and association reactions markedly lengthened in advanced age, while there was evidence to show that the time it took to calculate simple problems, in other words the association time for well-rooted associations, did not suffer. The study of the nature of associations showed a greater poverty of ideas in the old.

For the study of *memory* in its widest sense various methods have been devised. Every psychiatrist has certain rough tests for this purpose and for ordinary clinical observations we find in Sommer's book excellent tables and analyzed examples. The memory for old events is of course scarcely amenable to systematic investigation, owing to the wide differences in training and experience. But studies of the ability to register impressions are not only possible but, as results show, very promising. Thus the method of memorizing figures or senseless syllables has been used to study the effects of certain drugs, and with this method it was found for example that bromide of sodium and fasting affected the memory especially. We may also mention the method of Finzi which we described under the heading of apprehension. For the purpose in hand it was modified, inasmuch as variable spaces of time were allowed to elapse between the seeing of the exposed card and the giving of the result. With this method Rüdín, for example, found memory changes in alcoholic intoxication.

Schneider studied the ability to register impressions in senile dementia, a condition in which, as clinical experience shows, the memory for recent events is especially affected. A simple method was used. Patients were shown objects or pictures which they were asked to name. After intervals of 5, 10, 15, 30, 45 and 60 seconds they were asked to tell what they had seen. Sufficient experiments were made with each patient so that for each interval 100 tests were collected. The total correct answers varied from 37 to 72 per cent. The patients remembered chiefly by word images. Interesting is the fact that the diminution of correct answers did not go hand in hand with the length of intervals, but for each patient there was found a certain point, after which the answers became suddenly much worse (for example, between 15 and 30, or between 30 and 45 seconds). S. regards this as probably due to the fact that at that point the after-effect of the sense impression fades and that consequently the effect of distracting influences can have full sway. On the other hand, certain facts spoke in favor of the view that the development of greatest clear-

ness of the images, which according to Finzi takes place in the normal at 15 seconds after the impression, is much retarded. The method is not essentially different from the ordinary tests which we use in our clinical analyses. Nevertheless it yielded some results which the latter did not give, and it is moreover of great value to have more extensive investigations made in a systematic manner.

Ranschburg devised the following method for the study of memory. (1) He gives to the subject 15 pairs of words, the two words of each pair being associatively related according to (a) habitual word combination, where, however, both individual words by themselves have a meaning, such as door-knob, steam-engine, (b) according to co- or subordination, time or space coincidence, etc., such as hand-finger, fish-water; and (c) associations according to sound. In the test only the first word is given, while the second had to be added by the subject. (2) Among 50 photographs four men, four women and two children are selected and shown to the subject. In the test these have to be picked out. (3) Strips of five different colors are shown. In the test these have to be picked out among 15 different strips which represent five colors but for each there are three shades. (4) Among 693 squares of a card 150 are filled out. Among these, five are pointed out to the person and are in the test to be picked out again. (5) Five pairs of words are given, but in contradistinction to (1) they are not related. The test is made as in (1). (6) Five photographs are shown and names given with them. In the test, photographs are shown and the subject is asked to give the names. (7) Numbers are given, but in order to make it more like what occurs in daily life (a principle which we see is followed in all the tests), the numbers are combined with either a month, or a street, or the like, as September 17, 25 pounds, etc.

The entire series is so arranged that the first four groups are given in succession. Then follows the test for (1) and (2), then the groups (5), (6) and (7), and finally the tests for (3) to (7). Every correct answer counts one point, a wrong answer zero. Every mistake, however, is questioned at once and if corrected it counts a half.

With this method R. examined 12-year old boys, hospital attendants, educated adults, neurasthenics, and general paralytics. From the figures obtained, an idea of the extent of memory for all as well as for individual groups could be obtained. By calculating the percentage of the corrected in relation to the correct recollections, R. obtained another figure which he calls an index of the certainty of memory; the greater this percentage the weaker the 'certainty.' We must

admit that for simple tests the method is a very excellent one. As a result of his studies R. comes to the conclusion that education increases the 'extent' of memory; it increases the 'certainty' in some, but not in other fields. Age increases both. Interesting are his results in neurasthenia: there was a decided diminution of correct answers in the field of word memory; this is even more marked for senseless word combinations; the name memory is diminished somewhat, the person memory less; the 'certainty' is also diminished; besides, there was noted a greater proneness to fatigue and a certain slowness in reproduction.

In general paralysis there was marked diminution of 'extent' and 'certainty' of the total memory. So far as the special fields are concerned there was total or almost total destruction of the word memory, name memory, of the memory for localization of squares, while that for colors and persons was relatively well preserved. In certain cases, however, namely in early ones in which there was present a maniacal stage, the memory may be normal or 'even above normal.' Occupation has a marked influence in preserving memory in certain fields.

An important field for investigation promised to be the study of *motor processes*, and as a matter of fact this has been taken up from various sides and various methods have been developed.

Sommer and his pupils studied especially the involuntary processes, Kraepelin and his pupils the voluntary ones.

Sommer attempts, as he tells us, to study the innervations which accompany and in part characterize mental diseases. In this general plan he studied, *e. g.*, the reflexes and the inhibition of reflexes through cerebral influences. He devised an ingenious apparatus by means of which the reaction produced by a tap with a percussion hammer on the patella tendon (knee-jerk) could be multiplied and registered. For this purpose he supported the thigh above the knee, so that the lower leg could swing freely. By means of a string, which was attached at one end above the ankle, ran over a pulley, and at the other end bore a weight (which could be increased or decreased so as to place the lower leg at different angles), the swinging leg was 'equilibrated.' On the string above the weight was a recorder which wrote on a revolving drum. The force of the hammer could be varied and measured. It was found that the normal knee-jerk consists not of one rise only, but of a number of oscillations. With this apparatus he found marked changes in hysterical and epileptic patients, and also in katatonia, a mental disorder associated

with abnormal tensions in the muscular system. Thus he found, for example, unusual variations in epilepsy, irrespective of seizures, which were sometimes periodic and sometimes associated with many oscillations. In grave hystero-epileptic and hysterical states he found after three or four oscillations a renewed rise of the curve, in some hysterical and neurasthenic states an insufficient return to the base line, in katatonic forms signs of abnormal inhibitions. These are undoubtedly very valuable results.

With this method Alber studied the effect of alcohol and found that with increasing doses there appeared suddenly a point where the cerebral inhibitions were diminished as shown by an increase in the number of oscillations. Hornung modified the method by merely letting the leg fall from an extended position, instead of tapping the patella tendon. He found in epileptic dementia and dementia with hysterical symptoms ('*erethic dementia*') a diminution of inhibitions, *i. e.*, prolonged oscillations; the same was noted as an effect of alcohol.

Various methods were devised by Sommer for the study of movements of expression, for example one for recording on a revolving drum the horizontal and vertical movements of the muscles of the forehead. Results have not yet been published so far as I am aware. Another apparatus was designed for registering the involuntary movements of the fingers. The arm was supported by a freely swinging strap and the finger placed upon a small plate which was so arranged and connected that the movements in three directions could be separately recorded on a revolving drum. With this method he discovered periodic tremors, for example, in a case of epilepsy. Alber used this apparatus also for his study of the effects of alcohol. He found that alcohol produced at first a greater steadiness and then, at the same time that the loss of inhibition showed itself in the knee-jerks, a marked diminution of steadiness. There were also seen fine tremors, coarse lateral oscillations, and an increase of pressure. The greater initial and the lesser subsequent steadiness he is inclined to place in relation with the initial increase and the subsequent decrease in the facility of liberation of motor impulses demonstrated for alcohol by Kraepelin and his pupils.

A similar apparatus which Ermes used for the study of katatonic conditions was made for the leg. The leg was held extended and in E.'s experiments the lateral and vertical motions were recorded. He found an unusually long retention of the same level but also signs of fatigue in the form of increasing tremors, hence nothing that would really

add anything to what can be observed clinically in cases with cataleptic phenomena. He attributes the phenomenon of catalepsy to a narrowing of the field of consciousness and a consequent perseverance of cortical innervations in the absence of displacing influences, — a view which is in some ways attractive.

As has been said above, the study of voluntary motions was undertaken by Kraepelin. After various experiments with reactions, especially the 'faulty' (too precipitate) reactions found under certain conditions (after alcohol, bodily exertion, exhaustion due to keeping awake), had pointed to an increased facility of liberation of central motor impulses, Kraepelin in order to study this and other alterations more accurately devised his 'writing balance,' basing his idea upon a cruder apparatus of Goldscheider. A small desk upon which the arm can rest naturally contains a plate upon which a writing card can be fastened. This plate which is movable in a vertical direction, represents one arm of a balance, the other, a longer arm, is connected with a recorder which writes on a revolving drum. The latter arm is held down by a tight spring. During the process of writing the plate is depressed and the spring becomes stretched; if the plate is at rest the spring is relaxed. The subject may be asked to write the figures 1-10, or a German M, or to connect two points by a straight line. The tracings on the smoked drum can then be studied. The pressure can be measured and variations noted. The abrupt or gradual beginning or ending of the curve produced by each figure gave an indication of the rate with which the motion commenced or terminated. The size and form of the movements could be seen on the card. The size could be measured in mm. (at first this was done only for the straight figures, the 1 or the M [Gross], later a curvimeter was used [Diehl]). From the distance covered by a letter (in mm.) and the time it took to execute the movement (as measured on the drum) the relative rapidity could be calculated.

With this apparatus Gross, who was the first to describe it, made studies on normal persons and on cases of manic-depressive insanity.

It may not be out of place to say a few words about this mental disorder. The group of manic-depressive insanity includes non-deteriorating cases, which show a tendency to recurrent attacks. These manifest themselves either as states known as mania or melancholia or as peculiar combinations of these syndromes ('mixed states'). The manic state is characterized by a motor excitement, 'flight of ideas' and exhilaration; the depressive state by a motor retardation, a slowness of thinking and an emotional depression. While formerly

these states were considered to be the very opposites, Kraepelin has demonstrated their fundamental relationship. One of the proofs for this is the fact that these fundamental symptoms may be variously combined.

It was natural that these conditions above all others should tempt one to study the alterations in the voluntary motor processes. This Gross has done after establishing normal data for comparison. He found that in cases with motor retardation the rapidity of motion and the pressure were diminished, there was a gradual beginning and ending of the pressure curves, which were low and drawn out. The pauses between the individual figures were in two cases not, in one markedly, increased. The latter points to a retardation in the transition from one motion to another. The letters were smaller. As the work went on the figures became smaller, while the pressure remained the same and the pauses showed a further increase in length. In the manic condition G. found large figures and sometimes increased pressure from the first, but the most striking feature was the change which occurred during the work. In the beginning there may be a diminished rapidity of motion, but this rapidly increases and with it the size of the letters, the pressure becomes greater; the pauses, at first not shortened, now become more brief, the work gets careless, the variations in pressure more marked, and there develops a precipitate beginning and ending of the pressure curves. These results are very interesting as they show that there exists not so much a motor excitement as an increased motor excitability. This, to be sure, may be seen clinically, but these experiments have fixed and emphasized it much more clearly. Finally some of the states were examined in which there was a combination of the syndromes. One patient presented essentially the traits of retardation. Others showed a retarded beginning which gradually passed into signs of excitement. Thus the most striking case showed at first low curves with gradual beginning and ending, with long pauses; progressively the pressure increased, the pauses became abnormally short, the curves irregular, and with abrupt terminations. It is certainly of great importance that Gross has here demonstrated a combination of motor traits which seem logically to be the very opposites, but which are here physiologically closely related. It is to be hoped that these experiments will be continued with other combinations of manic-depressive insanity, because it is these combinations which seem to be the most promising in giving us an insight into these interesting, yet in many ways still obscure, phenomena of this disease.

Owing to certain similarities which in some cases exist between what we may call malignant states of excitement and depression (*i. e.*, deteriorating states which are now to a great extent classed as dementia præcox), and the benign forms just spoken of, it is interesting to note that with cases of dementia præcox G. found a striking inconsistency and apparently a lawless increase or decrease of the different factors, as well as evidences of sudden 'blocking' of the impulses, all of which results are quite different from the harmonious regularity of the curves of manic-depressive insanity.

The same method was used by Mayer for the study of the influence of alcohol, in which, as we have above stated, the result of the choice-reactions had also pointed to a transient increase of psychomotor excitability. M. found with the writing balance a transient shortening (not very marked) of the pauses, and a transient increase of pressure, while the rapidity of writing was diminished (exclusive of pauses). This was followed by longer pauses, while the pressure fell. The rapidity was not influenced and the variations in pressure were retarded and diminished in number. Consequently M. found in alcohol also a temporary increase in the motor excitability, but it lacked certain traits of the manic state. This, as well as what is to follow, may well show the fine differentiation which is possible in these experiments.

Other states which have been studied are those produced by mental and physical work. Bettmann demonstrated that while the influence of a brisk walk agreed with that of mental work (adding of figures) in so far as it depressed the rapidity of simple mental processes, it differed in its effect upon the motor processes. His results with choice-reactions showed an increase in the 'faulty' (too precipitate) reactions. Mental work on the other hand not only leads to a result in the opposite direction, but may even counteract an existing increased facility of motor liberation. Miesemer repeated these experiments on the writing balance and showed that a brisk walk diminishes the pauses, increases the rapidity of motion, the size of letters, and the pressure, accentuating also the variations in pressure. This result agrees with Bettmann, also Oseretzkowsky and Kraepelin, who found an increase in pull number in the ergographic curves written after physical work (see below). After mental work, on the other hand, M. found an increase in the length of pauses between the letters, slower motion, a smaller size of letters, and a diminution in pressure. It will be at once apparent that these results agree much more with those of manic-depressive insanity than do those found in alcohol intoxication. This seems to me of interest, since the benign, non-deteriorating manic-

depressive states appear to have, more than any other mental disease, their normal prototypes. It may be added here that Miesemer found also motor states each characterized by different combinations as a result of the anticipation of the 'pleasant' walk, and the 'unpleasant' task of adding, and still another was found when the subject had to pass from an easier to a more difficult task. While the ultimate analysis of these states is not yet clear, the fact that differences can be demonstrated is of great value.

It was to be expected that the ergograph should also be used for the study of voluntary motor processes. Mosso's apparatus having been modified, Kraepelin and the writer undertook to study the effects of caffeine and of the ethereal oils of tea separately. It was found that the former improves the work and that this improvement showed itself entirely in an increase of the heights of the individual pull, while the ethereal oils had the effect of lowering the numbers. The opinion was expressed at the time that the pull-numbers were the expression more of the condition of the motor centers, while the height was the expression of the state of the muscles. It certainly seems that conditions which facilitate the liberation of central motor impulses, as has been demonstrated for example by Oseretzkowsky and Kraepelin in the case of alcohol, affect the pull-number, while caffeine, which is known to act upon the muscles, affects the pull-height. We shall presently see that the retardation of manic-depressive insanity also affects essentially the pull-number. We may therefore say that so far as our experience goes, changes in the central motor responsiveness show themselves, in ergographic experiments, in alterations of the pull-number.

We may add here the study with the ergograph which the writer made in cases of manic-depressive depression. As is the case in many of these studies, the problem arose from the question of differential diagnosis. The melancolias of manic-depressive insanity are characterized by a retardation of motion, as has been demonstrated so well by Gross. There are, however, cases in which this retardation is totally absent so far as clinical observation goes. Whether it could have been demonstrated by means of the writing balance I do not know, but it seems questionable whether the alteration was sufficiently marked to go beyond the normal breadth. It was hoped that by means of the ergograph a method could be found in which, owing to the great resistance which the weight offered towards voluntary motion, even the slightest changes might be demonstrated. The method was the following. For several weeks three exhaustion

curves were pulled on the machine every morning, each curve being separated from the other by an interval of 15 minutes. In normal individuals it was found that with this interval the second curve is often higher than the first (warming up), the third again lower. From day to day the practice showed itself either by parallel rise of pull-number and pull-height of the first curve, or by a rise especially of pull-number. In the cases of melancholia which were studied these conditions were altered. The practice curve only showed a rise in pull-height, while there was a progressive rise in pull-number in the three successive curves on an individual day. In a more pronounced case practice as well as 'warming up' were absent. The explanation which seems to be the most plausible is that there exist resistances, especially towards the initiation of motor impulses, thus affecting the pull-number, while, the initial resistance being overcome, the rest of the pull (pull-height) is less influenced. In the mild cases studied, however, there was a distinct tendency towards an overcoming of these resistances as the work continued, which showed itself in the progressive rise of pull-number in the three successive curves. Interesting is the fact that these changes were found not only in depressive cases, but also in a 'mixed state' in which there was a slight exhilaration but no overactivity. It was therefore possible to demonstrate with this method in mild cases the same motor retardation which manifested itself as a more pronounced disorder in the graver cases, and, since the slightest retardation is associated clinically with a characteristic feeling of inadequacy, our results give to this a special significance.

We now turn to the domain of *feelings* in which Ragnar Vogt has given us the first, and thus far the only, experimental investigation. V. bases his work on the well-known studies of Lehmann and was able to use Lehmann's original plethysmograph. Several points which Lehmann had discovered were confirmed by V. and found to be much more pronounced in his pathological material. Thus Lehmann's claim that sudden changes in the arm volume are due to external impressions or arising ideas while the gradual changes are due to vague mental processes; for example, in patients who had delusions about the apparatus the sudden variations were very frequent. V. also confirmed Lehmann's finding that an expectant mood and a state of tension in general was associated with a small arm volume and a low amplitude of the pulse. For example, when in the beginning of the experiments some of his patients were uncertain as to what was to come, both volume and pulse were low, but rose when the patients grew accustomed

to the conditions. Of especial interest is the fact that he obtained two different plethysmographic pictures in states of fear, namely either a low volume and low pulse or a high volume and high pulse. In both cases the frequency of the pulse was increased. According to V. the difference depends upon whether fear is associated with tension or not. In one case we are dealing with an expectant fear (small volume, low pulse), in the other case with an immediate, or, as it were, unreflective fear (high volume, high pulse). He found that the former was present when in his experiments he only spoke of hurting the patient, while the latter appeared when a needle was brought close to the eye. He also met with the latter in imbeciles, whom he takes to be unable to concentrate their attention upon what will happen, but to be overwhelmed, as it were, by impressions. Differences were also noted in the attention reactions; sometimes the reaction agreed with Lehmann's typical results, but at other times it was associated with vascular dilation, namely, when, with the task of calculating sums, the patient experienced a feeling of bewilderment or shame about the result. If to this was added doubt or anxiety about having wrongly calculated, which was sometimes so marked that the patient hesitated to utter the result, it was associated with greater frequency of the pulse beats. V. points out how states in which the attention is directed ahead and strongly concentrated differ in the plethysmographic picture from those 'reactive' states in which the attention is diffuse (relaxation). V. certainly has shown how this difficult field in which psychiatry has many problems may be fruitfully attacked.

One more paper should finally be mentioned, namely that of Wizel, who studied with simple tests the ability to estimate time and space in cases of general paralysis and dementia præcox. He found most pronounced deviations from the normal, especially in the former, and attributes to this the well-known extravagance and absurdity of the delusions in these cases.

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PSYCHOLOGICAL LITERATURE.

Allgemeine Anatomie und Physiologie des Nervensystems. ALBRECHT BETHE. Leipzig, G. Thieme, 1903.

This extremely interesting and important contribution will be reviewed more at length in the *Journal of Comparative Neurology*.

Bethe's book, like Loeb's, is essentially a connected statement of a remarkable series of experimental studies. The greater part of the work refers to the peripheral nervous system. Above all we mention the demonstration by Bethe of the possibility of independent regeneration of peripheral nerves, limited to young animals. His evidence is very carefully collected, and the splendid series of experiments goes a long way to make the conclusion final. Whether it is necessary to infer from this that the development of the nervous system in the embryo must be the same as what happens when the abnormal conditions require atypical regeneration, is a wholly different question, consequently the general conceptions of the nervous system with a broad use of the concept of the neurone would hardly seem to lose its justification and usefulness.

Bethe's experiments show that function and trophism are considerably independent, and even the concept of trophism, that is, the life-regulating function of the nucleus of a cell unit, must evidently not be taken in too abstract a sense. Bethe shows that a nerve fiber cut from the cell does not degenerate in its whole length at once, but that the degeneration progresses from the point of lesion to the termination. Therefore, the concept of trophism must be extended so as to include what Bethe terms a polarization. In nerve fibers which are regenerated independently the same law of polarization holds as in the normal nerve tissue, that is, if a regenerated nerve is severed, the degeneration also spreads to the periphery. The products of this autogenetic regeneration in young animals disappears again within about ten months, if there is no union with the central nervous system. This finding, together with Bethe's experiment on the crab (see my review of Nissl's book), would show that the cell concept is unnecessary for nervous function as such.

The most interesting finding of Bethe is his demonstration of the direct or primitive stainability of the fibrils of the nerve elements, as being absolutely in harmony with the degree of functional conductivity. By pressure he was able to reduce the excitability of the nerve, and

found that the reduction was absolutely parallel with the reduction of stainability. With the same principle he was able to demonstrate histologically the conditions of anelectrotonus and catelectrotonus.

A number of facts allow Bethe to specify two chemical substances of fundamental importance, namely, the fibrillic acid which is at the bottom of the direct stainability of functioning nerve fibers, and the Nissl acid contained in the stainable substance of nerve cells, which is the substance of the central nervous system which Bethe is inclined to connect with a specific inhibitory function.

An important point in Bethe's findings is the demonstration of a special peripheral nerve net, independent from the central nervous system, and present in the heart, blood vessels, etc. This type of the nervous system is of a character of what is seen in invertebrates. These findings strongly discourage the view of non-nervous propagation of muscular activity in the intestines and heart, as had been maintained for a number of years by most investigators.

A review cannot do justice to the wealth of well-planned experiments reported in the book, nor to the interesting reviews of general topics of inhibition, etc. These notes must suffice to draw the attention of the reader to the original, and to the longer analysis referred to.

A. M.

Die Neuronenlehre und ihre Anhänger. FRANZ NISSL. Jena, G. Fisher, 1903.

The cock-sure generalizations which have become the center of neurological teaching, since Waldeyer gave his definition of the nervous system as a concatenation of independent cell-units, have called for Nissl's very searching discussion, which starts out to prove that the neurone theory is effete and finally overthrown. Like all concepts, the neurone theory had become a dogma for some, and a starting point for development for others. Nissl's motto 'écrasez l'infâme' goes against the very heresy of the principle. He obstinately claims that 'neurone-theory' can never mean anything but what Waldeyer meant, the assumption that (apart from the unessential vascular apparatus and the neuroglia) anatomical units of nerve-cells made up the functioning mechanisms called nervous system, and that these units were identical with the Golgi-silhouettes, not as Golgi saw them, but as Cajal described them.

Nissl's book is an arraignment of a series of workers who have made themselves champions of the word neurone. In a long-winded and formally awe-inspiring argument and with the zeal of inquisition,

he wants to have it out with all the workers who do not agree with him and Apáthy and Bethe. Nobody would deny that Nissl has laid bare a carelessness of expression and reasoning in many famous lights of neurology, such as partly excuses his reproving attitude.

Nissl's book is a peculiar mixture of pedantic dialectics, and a consequently much-diluted stream of statements of fact. A review is decidedly worth making in order to concentrate the facts so that they can be surveyed.

Nissl distinguishes gray and white matter, and as independent elements cells, fibers and gray; cells 'without gray' may be seen in the white substance beneath the convolutions; on the other hand gray matter may be interspersed among fibers 'with but few cells.' The 'cell,' is a well-circumscribed entity consisting only of the cell-body as it is shown in the 'fever-alteration' by Nissl's method. It has its nucleus, and a protoplasm body with dendrites, in most types with stainable substance; fibrils can be shown with Bethe's stain passing through the body, from surface to surface and dendrites to dendrites, and many passing out through the axone-process (which is free of 'stainable substance') and as a fibril-wire beyond the real axone-process (where it is probably lost before it enters a medullated sheath, since the Golgi-method is no proof for Nissl, and he evidently is extra-scrupulous on this point).

The cell is surrounded by a reticular 'Golgi-net' except in the region of the axone-hill. The net is smooth on the inside but passing directly into the pericellular substance, 'the gray,' a ground substance not analyzed histologically so far and 'certainly' not made up merely of dendrites and fiber-terminations. Nerve fibers are either connected with the nerve-processes of nerve-cells or they originate outside of cells from the 'gray,' because their number is much greater than the number of cells (?). Bundles of medullated fibers never connect more than two gray centers (?). The axis-cylinder (as such he only admits the part of the fiber which shows a stainable stroma with Kaplan's stain) cannot be followed beyond the point where the medullary sheath begins and ends. Only the neurofibrils connect with the gray, and with cells. Between the 'cell' and the medullated fiber there is a gap not filled in (?), because the acute alteration and Kaplan's stain of the axis cylinder fail to bring out the 'fibril wire.' In other directions, where even the Golgi-method fails, he admits his own postulates: not only fibers without cells, but paths in the gray which connect, for instance, the proper pyramidal fiber with the proper motor cell. Nissl doubts that the 'gray' should be a three-

dimensional diffuse net, as Apáthy claims. The central gray must be not only a nervous conductor, but a participant in nervous function. Like the intra-cellular and intra-neural fibrils, the gray is the 'highest form of differentiation of living matter,' with a metabolism of its own, by no means necessarily trophically dependent on nerve-cells. It is a postulate that the gray of various regions of the nervous system is far from identical in function. (In the older literature structural differences of the 'ground-substance' were well enough recognized, as is shown by the terms 'gelatinous' substance, etc.). It is a non-cellular, specific nervous substance, with provisions for localized conduction and capable of bringing into effect nervous activities of the most varied kind. Its nature is not settled, not even in the case of the neuropil, the point-substance of invertebrates, described as a 'gitter' by Apáthy; it is not certain that his fine net is identical with fibrils. Here, Nissl refuses to make even an hypothesis. He merely claims that the Golgi nets are an accessory apparatus of the nervous tissue which is the intermediary of the formation of conducting fibrils out of the constituents of the nervous gray.

Such a mixture of punctilious accuracy and idiosyncrasies, and freedom with hypothetical stipulations!

Beth's frequently mentioned experiment on the crab is interpreted by Nissl, and he introduces a very remarkable blunder. The cell-bodies are cut off and with them the perinuclear net and also the fibrils growing out from it as peripheral fiber or axone. Consequently for all motor purposes the cells are annihilated (and even their fibers would be wholly out of commission, even for Nissl). Since the reflexes reappear for about two days, Nissl assumes that special motor fibers originate directly from the neuropil. The first condition is that really no fibrils pass from the dendrites to the axone except through the perinuclear net and its efferent fibril. *This, according to Bethe, holds for Hirudo, but not for the crab* (pp. 98-99, and also Fig. 12, p. 35). Nissl's elaborate drawings which should refute such objections as mine in the *Journ. of Comp. Neur.* are made up of *Hirudo* cells for a *Carcinus* experiment! This, to my mind, ought not to happen to a man who is such a severe critic of others. The second condition — using Nissl's anatomical standard — would be the *demonstration* of fibers which go to muscles, and originate from the neuropil without passing through cells or parts thereof. Where is the evidence?

Nissl's chief wrath is expressed in the assumption that the 'neuronists' claim that nerve-cells and their processes should make up the

whole of the nervous substance. This, to my mind, has never been claimed as absolutely and irrefutably true, but merely hypothetically on the ground that it is best to start from the known, and not to assume new things unless profitable hypotheses lead us further. The attitude expressed in my extensive review of 1898 is a standpoint to which I have every reason to hold to this day, because it invites new facts and does not exclude any. The question might be raised whether the term neurone was a desirable term to deserve preservation after the numerous accidents of birth: an extreme definition by Waldeyer, and Shaefer's using it for the axone. Personally I am not inclined to be an extreme purist. With the proper definition and as long as the facts implied are plain, the term 'neurone' is useful, although like the word 'cell' not to be used very frequently.

The chief failing of the Golgi and Ehrlich methods is that they have been misinterpreted by some, and that they show more than the cells of Nissl's 'acute alteration.' In my review, I strongly pointed to Golgi's views as contrasting with Cajal's. Golgi claims a passing of the cell into a *réseau diffus*. Nissl prefers to say: My own method in pathological states shows cell pictures which alone can be accepted as cells; that which goes beyond is largely unknown, and must in part be called 'gray substance.'

Forel, as is well known, arrived at his views (which were not a dogma, but meant to be a simplifying correlation of formerly disconnected facts), from his experiments with the Gudden method and the Golgi-pictures. Nissl's peremptory claim against this direction of argument is that Waldeyer's theory is an anatomical theory and can only be explained anatomically; and that the theory does not *explain* the fact of circumscribed degenerations, as long as the details of the events in the ground substance (Nissl's gray matter) are unknown. The limitation of evidence to 'anatomy' is sophistic. Nissl never adheres to it himself. It reminds me of the stubborn claims that mind can only be explained by mind, etc., and rulings of court-evidence, not of natural history logic. In the problem of degeneration, the neurone-theory is a convenient frame for many of the known facts, nothing more; there is no attempt to explain beyond the facts, but to formulate hypotheses to be tested in the light of fact. He also repudiates the value of a term like 'trophic neurone' because it is 'practically nothing more than another way of expressing the results of secondary degeneration and the results of the Gudden method, and not an explanation of these.' Such claims have of course been made; but they do not touch the question of fact, but merely the danger of simplifying presentation for certain minds.

The embryological researches of His are very haughtily treated by Nissl. Here again, he denies any weight to the embryological analysis, because it is not anatomy of the adult. He, however, introduces any evidence he chooses. The acute alteration gives his true cell-picture. He denies that the posterior roots arise from the spinal ganglia. His arguments concerning the effect of section of the posterior roots and the denial of their outgrowth from the spinal ganglia are experimental. He finds changes in a few cells of the posterior horns, but none in the ganglia, in harmony with what has long been known. This simply means that 'Nissl's law' of axonal alteration is not all-embracing, and would be a premature generalization. Why does he not draw in the inconvenient fact that in 'amyelic monsters' the spinal ganglia send posterior roots into the empty spinal canal (Leonowa)?

Apáthy's findings are a very interesting addition to our knowledge of details of the nervous mechanisms. They show that the concept of nerve-elements of vertebrates must not be generalized. He demonstrated neurofibrils and fibril-nets within the cells, and fibers and fibril-nets forming the neuropil. Bethe has added some facts for vertebrate neurology, calling for suspense of generalization, but without furnishing a sufficiently full picture of the organization to replace the present broader concepts. Between Bethe and Nissl there are considerable differences of opinion, and it is well that *no fundamental decision should be claimed to exist until a larger number of workers can agree on ground of personal experience*. Until then the larger facts will have to decide the general tone of didactic presentation.

The world is a very complex medium. Whoever wishes to move through it successfully must, by instinct and training, make the best of those chances for friction which *steady* the course of life, and glide along as smoothly as possible over the points of useless friction. Success is at best a compromise. To intentionally multiply points of friction, may do for the strong and untiring pioneer; but to give others a start, a sound choice of essential helps, and faith and an open eye for more helps, is necessary to avoid fumbling and disorder of habits. In this direction, a broad neurone-concept has its sphere.

In my Critical Review of the Data and General Methods and Deductions of Modern Neurology (*Journ. of Compar. Neurology*, Vol. VIII., 1898), I pointed out many open questions which should keep the neurone concept free from dogmatism. The following year (Vol. IX., pp. 38-45), I showed why Bethe's experiment is in line with our experience in vertebrates and not a contradiction. Since then Bethe's

evidence of autogenic regeneration of nerves has substantiated some claims considered in 1898. With all this new material I should feel tempted to reprint my comments of 1899 in full.

Nissl's book is far from doing fair justice to all the facts which enter into this question; and, in order to defend his chosen position he makes the most of the great number of gaps worth bearing in mind. He would have done greater service to the neurological world if he had admitted more broad-mindedly the facts of physiology and pathology, at least for what they are worth. He would then have been less in need of using unfounded hypotheses to make up his own hazy scheme of the nervous system.

Nissl does not repudiate hypotheses as such; but his great law says: as soon as one fact is established which contradicts the law, the theory must be dropped;—I should add, if it has not enough vitality to remain profitable when it is adapted to improved knowledge. By ignoring well-established facts, by misrepresenting Bethe's 'fundamental experiment' in borrowing the cells of a leech for the crab, by claiming more for his 'gray' than even Golgi claimed for his *réseau diffus* (to whom, by the way, he does not give credit), by distracting the innocent reader with arguments which would at once become unnecessary if he furnished a series of conclusive photographs of products of adequate experiments, and by exploiting the opportunity of showing the slips of his fellow-workers, Nissl makes an impressive plea for his thesis. He will have many followers. May they be among those who cannot get over *misusing* the neurone-concept as a cover of their ignorance of actual facts.

A. M.

Die Schrift bei Geisteskrankheiten. Ein Atlas mit 81 Handschriftproben. Dr. RUDOLF KÖSTER. Vorwort von Prof. Dr. R. SOMMER. Leipzig, J. A. Barth, 1903.

As part of the programme of Sommer, who aims to develop a system of methods of objective and accurate observation, this atlas of 81 samples of hand-writing is a supplement of the book on *Diagnosis of Mental Disease*, in line with Alber's Atlas of photographs of patients. The samples are well chosen from 41 patients. Each case has a brief summary of the facts on which the diagnosis is based, and an analysis of the samples: general paralysis (with ataxia of motion, omissions and repetitions and evidence of mental deterioration); cerebral lesions with agraphic disorders (fairly correct in execution, but with paraphasia or agraphia according to localization); multiple

sclerosis (very slow ataxic execution), in one case with peculiar mistakes; senile dementia (awkwardness with attempts at correction), is compared with ordinary senile tremor; delirium tremens. With the states of confusion, epilepsy, mania, catatonia, paranoia, etc., the arrangement and the contents of the writing become more and more characteristic as compared to the mere form.

The book aims to keep aloof from all graphological discussion. In the preface Sommer advises not to formulate the type of disorder dogmatically, but to make the findings appear as part of the general diagnostic task in definite cases. He especially draws attention to the absence of any changes which are supposed to be characteristic of anxious agitation. A brief introduction by Köster gives a survey of the literature.

No investigation with analyses by curves are given.

It is quite evident that samples of writing are extremely important documents, but that they should be used merely as part of the material in the entire clinical setting, and not for snap-diagnoses.

A. M.

Die Sprache der Geisteskranken nach stenographischen Aufzeichnungen. Dr. med. ALB. LIEBMANN, und Dr. med. MAX EDEL. Vorwort von Prof. Dr. E. MENDEL. Halle, a. S., C. Marhold, 1903.

Much more than Köster's book, this work raises the question of the value of symptomatological analysis on ground of principles extraneous to the issue of the establishment of evidence of distinct types of abnormal working. Morselli's *Manuale di Semiotica delli Malattie Mentali* (Milano, 1884 and 1894), a monument of zeal and good observation, is probably the most extensive effort at a description of what may occur in insanity, and may be grouped according to essentially logical principles: expression, external aspect, attitude, physiognomy and mimic, language, writing, conduct (general and special dyspraxias); then the analysis of consciousness, intellect, sentiment and will. Many text-books have an introductory 'general psychopathology' with an order borrowed from various psychologies; Kraepelin, and still more Ziehen, give such systematic presentations of symptom types. The question naturally arises, What is the advantage of an analysis which splits up events according to the mere appearance of detail? Symptoms mean something as part of a whole clinical complex or as evidence of the abnormal working of part of our biological regulations or mechanisms. The lengthened descrip-

tion of a special symptom means nothing or is directly harmful by distraction, if it does not advance either our knowledge of the working of a definite mechanism, or some relation in a whole complex, whether we know all the details about the working of the mechanism or not. Between these extremes, the evidence of elementary disturbances and the evidence of more or less roughly empirical characterizations, there are innumerable forms in which language or any other reaction may enter into the course of events without playing an intrinsic part, and to accumulate material on these fruitless fields is to accumulate raw material, probably without any value unless presented in its entire setting.

The authors study spoken language, for mechanical changes of sound formation, sound connection, sound sequence, tempo, and character and strength of voice, and for formal changes (odd contents, queer expression, deviations of syntactic and grammatical forms), and as a frame they use the nomenclature of the Prussian statistical bureau. Statistical classifications are bound to be compromises and what is put under one heading here is apt to be viewed quite differently by most alienists. The groups are:

I. Simple psychoses, divided by the authors into melancholia, mania, acute hallucinatory confusion, chronic paranoia psychoses of adolescence (*dementia præcox*, *hebephrenia*, *catatonia*, *stupor*), secondary dementia, senile dementia, and a few organic disorders — multiple sclerosis, and cerebral syphilis.

II. Progressive general paralysis.

III. Psychoses with epilepsy and hysteria and with imperative concepts.

IV. Imbecility and idiocy.

V. Intoxication psychoses: alcoholism, especially delirium tremens; morphinism and cocaineism.

Under each heading a brief summary of the cardinal symptoms of each disease form is offered, extremely schematic and fragmentary. Special disorders of language are then referred to, naturally without any reference to distinctions not implied in the vague definition, with instances and opinions selected from various writers; and this is followed by stenographic samples of utterances of various patients. They are introduced with the most meager statements; only in a few cases are the general setting of the conversation and the course of the disease indicated. The general statements are mostly anthologies from the literature, and the really well chosen and frequently interesting stenographic samples of utterances, are practically without analysis,

and the many questions to which they give rise in one's mind are not answered.

Liebmann is a specialist for disorders of speech, obviously without any but local psychiatric interests; Edel, the physician of the institution in which the records were taken. Their book is, I believe, the best instance of work on symptomatology as it ought not to be. For a general discussion of psychiatric symptomatology, symptom-complexes in the light of course and outcome, *i. e.*, empirical entities, should be the starting point; any abstract, however systematic the psychological scheme, splits the facts into merely formal elements, and the same holds for an isolated consideration of speech alone, or writing alone. The value of all detail work depends on whether it is in line with a natural setting. For this, neither the modified Prussian scheme of classification nor the splitting off of spoken language from the rest of reactions of a patient proved to make a favorable and sound ground. Had the writers given as good a summary of the setting of each case instead of the unnecessary abstract of definitions of obsolete general terms, the book might have gained much.

A comparison with Wernicke's 'Krankenvorstellungen' shows that even the technique of presentation of the talk of the patients is far from being equal to existing samples.

A. M.

Le langage intérieur et les paraphasies. G. SAINT-PAUL. Paris, Félix Alcan, 1904.

The results of over twelve years of attention to the speech-mechanism of thought is here put together. Largely with the questionnaire method, the writer has collected records of types of thought forms. As was to be foreseen, the types are varied and numerous: of 240 returns, he found 31 auditory (type Egger), 15 motor (type Stricker), 14 visual (type Galton), 98 auditory-motor (78 of which belong to Saint-Paul's motor type with secondary auditory reaction), 41 visual-motor, 3 auditory-visual, and 38 non-determined or indifferent. The author follows very closely a very simple scheme of localization and connection of the various centers: the visual, auditory, graphic and verbo-motor centers are connected with one another and with a 'center of ideation.' In the discussion of paraphasia he distinguishes paraphemic with or without realization of the blunders, the former an interruption between the Broca-center and the psychic centers or the leading center to which the motor center may be subordinated; the second form with an alteration of the connection between Broca-center and the incito-motor and motor centers

(p. 247). In the sensory paraphasias, after first showing the necessity of using not only usual words, but also non-sense syllables for the tests, he similarly defines 'aprojective' paralexia: loss of conscious recognition of words read, with possible preservation of a reflex recognition or reflex reading and copying of words read — a break between the psychic center and the center of sensory memories. This would even allow a transposition from print into writing. 'Aprojective para-word-deafness' is the possible preservation of reflex recognition and reflex reproduction in speech and writing of words heard but not understood by hearing. The projective sensory paraphasias (a break between memory and perception center, but integrity of connection of memory and psychic centers) imply loss of the recognition of words read or heard, with preservation of the mental projection of words thought, pronounced or read or written, but without reflex reading or transposition of print into writing.

The discussion of conduction-aphasias brings out clearly the scheme of types of endophasia, but also the lack of sufficiently analyzed cases which would not seem to be arranged *ad hoc*. The rôle of 'leading centers' evidently varies much, and it will take much persistence to develop plans of examination in each case, for the purpose of a careful study of the endophasic and aphasic changes in the patient, on which, after all, a system of reëducation depends. Especially agraphia and paragraphia remain a very complex individual problem difficult to bring under one formula.

In amnesias, Saint-Paul provides his scheme with independent paths from the psychic center to the memory center and from the memory center to the psychic center. Thus the centrifugal amnesias make impossible the rousing but leave intact the revival of memories (the patient understands, but cannot produce at will). In the centripetal amnesias the projection of the image on the psychic center is suppressed, the memories are not roused by impressions; the patient does not find names, and does not even understand them when he meets them. Where the projections work normally, but the rousing of memories and identification is inadequate and disturbed, we deal with true 'psychics,' alienation, imbecility, idiocy, intellectual weakness, and no longer with a mere amnesia.

A final chapter deals with the 'subnormal states' of dreams, hypnosis, somnambulism (Helen Smith), and finally deliria, intoxications and insanity generally, and promises, as a continuation of his studies, practical deductions (internal language and the art of rhetoric, the phenomena of thought transmission, and the study of languages).

The generalities in the first part of the book (anatomical correlation and method of work) show the immense difficulties of the proposition which the author has put before the reader. The simplicity of his diagrams is very commendable. The simpler they are, the better, as long as they have so little actual anatomical correlation. The attempt at psychophysical correlation with the conception of the 'fonction-miroir' of consciousness and the differentiation between endophasia and verbal memory forms an excellent exposé of matters to be borne in mind in the study of endophasia. For some time to come every worker will have to work out his own scheme to fulfill the absolute condition of further advancement of the theory of aphasia: a careful analysis of the working of the modalities of speech in aphasic cases with subsequent correlation with accurate anatomical studies. This task still seems too great to attract many neurologists to more than a superficial study; but this work of Saint-Paul, that of Wernicke, of Pitres, of Pick, of Bastian, and others is bound to overcome the unsatisfactory schematic theories of the present day.

A. M.

Ueber das Primärsymptom der Paranoia. DR. JOSEF BERZE.
Halle, Carl Marhold, 1903.

The difficulty of the paranoia-question could hardly be better characterized than by what Berze has to exclude owing to the rigid adoption of Neisser's definition. "The main clinical feature consists in chronic delusion-formation, continuing from the beginning to the end throughout the entire course of the disease." He eliminates the cases of 'originäre Paranoia' (dating back to the earliest childhood), of psychopathic inferiority (casual paranoid development owing to excessive affects or domination of definite concepts), the cases of residual paranoia (with a transitory plus of symptoms at the beginning), cases in whom acute or chronic disorders, which have nothing to do with true chronic paranoia, call for lasting explanatory delusions, further the cases of neurasthenia, hysteria and hypochondriasis with paranoid conditions, unless he 'recognizes their truly paranoic character'; further, secondary paranoia (although in some cases a connection of dementia præcox with the subsequent chronic delusion formation in the sense of paranoia cannot be refused altogether), cases of Griesinger's general paranoia or dementia, and of course all the cases of symptomatic paranoia. Berze dispenses with the report of his case-records, 'because they add little to the plausibility of the author,' since the statement may easily be influenced by suggestive questions,

and because the usual anamneses do not consider the points of special interest to the author. It would seem that the desire for communication of the material should not be shaken for such considerations. Without the report of the cases it is difficult to see what is the proportion of direct observation and of inference in the author's work — a matter of considerable interest in such a complex field as psychiatry.

After a very brief review of the leading opinions on the origin of paranoia from Westphal to Störring, Berze asks: Will the chief representatives of the claim that paranoia originates from *affects* really convince us, or is the preference for such an explanation chiefly due to the dissatisfaction with existing intellectualistic theories? The strongest advocates of an affective, not intellectual, origin of paranoia, are Specht and Marguliés.

Specht's view is that emotion is the very center of the personality, that it has but two fundamental qualities, pleasure and pain; that the delusions of paranoia, too, show always a mixture of two cardinal directions, of persecution and exaltation; these two are the necessary compound which will create the attitude of suspicion, and, as a sort of counter-test, he claims that this affect and consecutive delusion formation invariably arise when states of depression and of exaltation blend, as in the transition period of circular insanity (?). He leaves unanswered the question, what produces this peculiar specific mixture so as to determine paranoia. Circular insanity would certainly prove that paranoia need *not* follow such an affect.

Marguliés assumes that a paranoic individual has a perfectly orderly intellectual life to start with. It would be difficult to see how a certain trend of ideation would become faulty by itself, unless the first disorder were emotional. He therefore assumes that under certain predisposing conditions some important event calls forth affects and a certain unrest, which lead to hypochondriacal concepts and morbid self-reference; these morbid states next lead to suspicion and apprehension and the ground for persecutory delusions is furnished. The plausibility of such an explanation and the difficulty of other explanations is, however, no sufficient evidence of the necessary conclusiveness of this picture. It remains impossible to prove that an emotional disorder must necessarily be primary.

The common initial condition is a certain sensitiveness, seclusiveness, and irritability. Berze expresses this as a rising of unpleasant feelings by events which attract the attention of the patient, while in a normal person, they would not annoy, either through the contents or through the way in which they command attention. The varia-

bility of the initial symptoms is due to the fact that the 'affect' is not constantly active, but merely temporarily produced in the beginning. The fundamental disorder is thus recognized as a *disorder of apperception, especially of passive apperception*.

For this view, Berze adduces some empirical evidence (the complaint of the helpless passive situation). The pathologically exaggerated feeling of passivity and the pathologically weakened power of criticism determine the development, partly because the person may be defective to start with, partly because the unpleasant feeling of passivity distracts. The paranoic person does not react as actively as a normal person would to similar actual experiences. Both the inquiry and the reaction are often suspended. There is an inability to digest and dispose of experiences, and many patients complain that certain thoughts take hold of them, sometimes in absurd but irresistible combinations. A patient with tooth-ache simultaneously had to think of the round shape of the earth, and finally had to believe that his tooth-ache had something to do with the shape of the earth (something that happens also in dreams and deliria — M.). The feeling of passivity leads to the idea of damaging influence, and in connection with the dominant ideas and the inability to dispose of them, morbid self-reference and ideas of persecution arise. The reduction of normal criticism leads further to grandiose ideas. Hallucinations are frequent but unessential symptoms of true paranoia. The psychic defect of the paranoic is a characteristic defect of apperception, not a dementia in the ordinary sense.

It is easy to see that Berze, by adopting Wundt's complex concept of apperception, with its necessary components of expectation, etc., succeeds in showing the onesidedness of the somewhat extreme incrimination of 'affects' as the foundation of paranoia. The pamphlet contains many good analyses; it furnishes a good formula of a complex happening; Berze also claims that on ground of this formula he can group together cases of a disease *sui generis*. Some kind of lesions of the apperceptive mechanisms (prefrontal lobe) are assumed; the nutritive attraction of the individual prefrontal cortical elements must have suffered so that the function of that part can rise with difficulty only to the level which is necessary for the integrity of apperception.

A. M.

Archives of Neurology from the Pathological Laboratory of the London County Asylums, Claybury, Essex. Edited by FREDERICK WALKER MOTT, F.R.S., M.D., F.R.C.P. Vol. II. Mac-Millan & Co., 1903.

The very creditable collection of studies is opened by Dr. Mott with 327 pages on 'Tabes in Asylum and Hospital Practise,' probably the best monograph on this topic. This is followed by a very noteworthy study of Amentia (Idiocy and Imbecility) by A. F. Tredgold, and by a study of the *Histological Basis of Amentia and Dementia*, by Joseph Shaw Bolton. It is this latter paper which shall be reviewed here more fully as most interesting from the point of view of possible correlation of cortical and mental efficiency in mental disease. Bolton examined 200 cases. He classed them in 5 groups: 33 without dementia, 52 with appreciable dementia, 51 with moderate dementia, 37 cases of dementia 'which still show symptoms of insanity,' and 27 cases of gross dementia. The clinical description is very scanty, hardly anywhere sufficient to allow of a diagnosis, but merely standardizing very roughly the amount of 'dementia.'

In each case Bolton looked for thickening or adhesions of the dura, presence of subdural deposits, excess of subdural fluid, the stripping of the pia-arachnoid (naturally, rather more readily than natural, readily, very readily, like a glove), excess of subarachnoid fluid, dilatation and granulation of the lateral ventricles and granulations of the lateral sacs of the fourth ventricle. In all these items (table of p. 471) the abnormality increases toward the fifth group. Thus the average weights of the pia are 17, 19, 25, 34 and 35 grammes: dilatation and even granulation of the lateral ventricle become frequent, and also granulation of the lateral sacs of the fourth ventricle.

The weight of the two hemispheres, the increase of weight through formalin-hardening, and the role of vascular disease are discussed; also the changes in the skull, the subdural deposits, the effect of gravity on the intra-cranial contents of the cadaver. The influence of heredity is spoken of on ground of statistics obviously too meager (in only 84 of the 200 cases a history was obtained and pathological heredity in 56 of these 84).

"Neuronic insufficiency is the necessary antecedent of mental disease." Underdevelopment and effects of stress and vascular degeneration and neuroglia over-growth combine. The introduction of dementia paralytica into the discussion makes the problem as broad as possible, to show that the *regions* of cerebral wasting are practically the same in all kinds of dementia.

Pages 506-545 are taken up by the discussion of general paralysis. According to Bolton paralytic dementia is *not* an organic disease of the brain, but a branch of ordinary mental disease, special clinical and pathological features being introduced in consequence of a former attack of syphilis. "It develops solely in the actual or potential subjects of those types of mental disease which, owing to a hereditary deficient durability of the cortical neurones, tend to end in dementia." 102 cases shall prove this heterodox thesis. He classes them as:

(I.) Dementia paralytica.

- (a) Juvenile dementia paralytica. (1) in imbeciles (usually chronic). (2) in high grade imbeciles who show mental symptoms about puberty (usually more acute).
- (b) Ordinary chronic dementia paralytica in higher grade degenerates.
- (c) 'Tabetic general paralysis,' [or dementia paralytica associated with extensive degeneration of lower neurones.
- (d) Acute or subacute dementia paralytica in the highest grade degenerates (general paralysis of the textbooks).

(II.) General paralysis without mental symptoms.

This standpoint in nosology and pathology forms a marked contrast to the views of Kraepelin and Wernicke.

The second part begins with a 'classification of mental disease.' Amentia (a term which has been introduced by Meynert in a totally different sense, and therefore is available only at the risk of confusion) includes the normal infant and patients suffering from deficient neuronic development: (a) All idiots and imbeciles, whether primary or secondary; (b) paranoiacs, or cases with fixed and systematized delusions who do not develop more than mild dementia; (c) cases of recurrent insanity not liable to develop more than very mild dementia, even in some instances in the presence of extreme vascular degeneration; (d) cases of chronic insanity without visible dementia, chiefly single women, of the dangerous, excited or 'moral' type; (e) cases of 'hysteria' in the widest sense; (f) cases of true epileptic insanity (see later). The second heading is 'confusion' attributed to direct or indirect action of toxines. In both amentia (at least the highest grades) and in confusion 'stress,' determines the onset. Dementia is, (a) Senile (dementia of worn-out neurones); or (b) dementia of degenerates who, owing to 'stress,' have become insane, the dementia is only moderate; in these the group embraces all the ordinary and the

primarily toxic insanities, from puberty to the climacteric period, with insufficient neuronc durability; or (c) dementia of degenerates which is associated with premature (as a rule) vascular degeneration following 'congenital' or acquired syphilis. Finally, the relation of epilepsy and insanity is stated; true epileptic insanity is a 'high-grade amentia.'

I give this brief abstract to show how different Bolton's psychiatric standpoint is from that of most alienists with clinical interests; an extremely vague and apodictic statement of constructions in harmony with the type of histories furnished in the first part, untouched by any work done by alienists in recent years. On such ground as this we are reduced to the principles of deficient evolution and of dissolution, to a statement of poor make-up and ill-health. It is quite obvious that the progress of pathology depends on the recognition of much more specific conditions, so that we can shape conclusive pictures of etiology, symptomatic development, course and outcome, and types with some definition.

The most interesting problem is approached on p. 553 under what Bolton calls the general histology of the cerebral cortex. In this he limits himself to the measurements of the laminae of the cortex in 20 cases: 3 normal adults (a woman of 38 who died of peritonitis, a deaf mute of 13 killed in an accident, and a woman of 36 who died of typhoid fever, and may have been below par), 5 'normal aments' (2 fœtuses of 4 and 6 months, 2 still-born children, and one child of 6 weeks), 4 'congenital aments,' 3 cases of chronic insanity without dementia (1 from group I., and 2 from group II.), one case each from groups III. and IV. (with marked dementia), two cases of gross dementia (group V.), and one case of gross paralytic dementia. Of these, case 9 is obviously an imbecile, case 10 a dementia from epilepsy (possibly traumatic, from the age of 9), case 11 also epileptic dementia in a child of 11, with convulsions since 5, up to which time she was evidently bright, but sleepless, etc. (*i. e.*, 'potential' ament?); at death she profoundly demented. The evidence of dementia in the records is very indirect; in case 16 and 17 it is emphasized by the remark: 'note the severe mental confusion.' The report of case 17 rouses suspicions that general paralysis might have been present.

The clinical evidence does not allow of any correlations with more definite principles of psychiatric diagnosis. The measurements are, however, firm ground, and point very interestingly to the special vulnerability or variability of *the second or pyramidal layer* which shows special underdevelopment. In general paralysis Bolton finds percentages of the layers returning to the condition existing at birth, 'allowing

for neuroglial and vascular proliferation in the two external layers'; in the prefrontal cortex of chronic insanity without dementia, underdevelopment of the pyramidal layer of nerve-cells exists, the other layers being approximately equal.

In his conclusions the fifth or polymorphic layer, which is the first to be differentiated, is said to be 'the last to fail in the retrogression,' 'in extreme aments and in demented who are unable to carry on the ordinary animal functions, such as attending to their own wants, etc.' "This layer, therefore, probably subserves these lower voluntary functions of the animal economy."

The third or granule layer develops afterwards; in the primary visual area it is the reception station of the optic radiation. "This layer, therefore, probably, reasoning by analogy, subserves the reception or immediate transformation of afferent impressions, whether from the sense organs or from other parts of the cerebrum." The second or pyramidal layer develops last and fails first. "This layer, therefore, subserves the 'psychic' or associational functions of the cerebrum."

This very rough sketch shows the possibilities of a relatively coarse method, far easier than the methods of cell-counts carried out by Hammarberg and recently by Lawrence, yet sufficiently accurate to make out differences. It is an interesting question whether sets of clinically well observed and well differentiated cases will lead to the same sobering results concerning greater differentiation in localization and disease-process in various types of disease. Bolton stands on an essentially nihilistic ground with his distinctions of amentia, confusion and dementia, and his records do not throw any light on improvements in psychiatric nosology.

The fact that he found interesting variations in the prefrontal regions should serve as a stimulation to investigate these *and* other regions of well studied cases. It would be easy to prove that his scale of vulnerability (prefrontal region, next the remainder of F 1 and F 2, next Broca's convolution and the ascending frontal, next T 1 and the parietal lobules, and finally the rest of the hemisphere as least vulnerable) has its exceptions, and not merely under vascular and traumatic influences. Taken as evidences of possibilities the paper is very suggestive and meritorious; but its conclusions demand a rather critical attitude.

Pages 621-724 bring a pathological-anatomical study of twelve cases of juvenile general paralysis by George A. Watson, a worthy counterpart of Mott's work. The coagulation-temperature of cell-globulin, and its bearing on hyperpyrexia, by Halliburton and Mott,

the prevention of dysentery and a number of other valuable studies, concern us less in this review than a contribution of W. G. Smith, on the 'Range of Immediate Association and Memory in Normal and Pathological Individuals' (pp. 767-805). Smith uses the method of immediate oral reproduction of series of letters. Somewhere between 4 and 10 letters the capacity of reproduction was found to break down; in others series from 6 to 12 letters could be used. He had to discard the reading from a typewritten slip, and the method with a shutter, and resorted to auditory impressions at the rate of 108 beats a minute. The results of the repetition are classed as: (1) rightly placed; (2, *a*) group transposed; (2, *b*) inversion in right position; (2, *c*) inversion in wrong position; (3) wrongly placed; (4) omission; (5) insertion; (6) repetition; (7) defect; (8) excess. The results allow of many inferences, but the method appears rather complex considering the issues to be covered in the examination of patients. Smith found that with healthy persons the range of immediate memory lies as a rule at 5 letters; the next addition produces a very decided fall in correct replies (40-50 per cent. on the average). With abnormal subjects the relations are less clear. In the normal and the abnormal the total of errors indicating partial dissociation or disorder remains relatively constant; no special fluctuation of attention or distinct improvement by practice is experienced. It is possible to differentiate with some precision between the more permanent memory and the power of immediate reproduction. There seem to be relatively distinct metabolic processes underlying the two activities.

To judge from my own experience, these studies have more general psychological importance than bearing on psychiatric problems. As a rule simpler tests are sufficient and conclusive, and they might easily be elaborated to lend themselves for graphic presentation.

The main criticism is that familiarity with the issues of clinical psychiatry and its problems would lead to more direct plans. Whether the elaborate apparatus of psychophysical methods and calculations will ever get a prominent place in psychiatry seems to me doubtful. For psychology proper, work on the abnormal is confronted with too many incalculable quantities; and the practical questions are usually met by simpler procedures.

The Archives are good evidence of the earnest work of Doctor Mott and his associates. It is to be hoped that it will get more support by thorough clinical work in insanity. In this direction the work of Bolton and Smith was not furnished the best foundation. The hospital authorities will, no doubt, soon fall in line with the movement and thus assist in the very creditable work of the laboratory. A. M.

Das Krankheitsbild der Apraxie ('motorischen Asymbolie') — auf Grund eines Falles von einseitiger Apraxie. H. LIEPMANN. Monat. f. Psychiatrie u. Neurologie, 1900, p. 15.

On Apraxia. H. LIEPMANN. Arch. f. Phychiatrie, Vol. 38, p. 300. 1904.

Liepmann has recently reported the autopsy findings of his famous case of one-sided apraxia. The importance of his observations is such as to warrant a review of the whole work at this somewhat late date. There is no contribution to neurology of late years that deserves so well to be put forth as a model of careful study of nature's experiments as this one. Nor can we point to any better instance of the remarkable influence of Wernicke's teachings. "That a man can act with his right extremities as if he were deeply demented, as if he understood neither questions nor orders, as if he could grasp neither the meaning of objects, nor the sense of print or writing, while he can show with his left extremities that all these apparently absent functions are present, has not been described so far."

The patient in question, born in 1852, an official with University training, had had syphilis about 1880, transitory fainting spell during the summer of 1899, and finally on December 2, 1899, a sudden attack which made him aphasic and produced a condition which for ten weeks was considered as one of 'aphasia with profound dementia following apoplexy' in one of the Berlin hospitals. He was transferred to a hospital for the insane.

The peculiarity was that the patient obeyed all requests which implied the use of the body as a whole; he would rise, would go to the window, to the door. When asked to pick up and show the use of some objects before him, he blundered in every attempt, acted perversely, and made odd movements with the right arm. *When the right arm was held* and he had to use the left, he correctly picked out cards which he could not do with the right; movements of the foot could be imitated with the left but not with the right foot. When the right side was inhibited the appearance of dementia was stopped and the patient could be examined. There evidently existed *motor confusion and perplexity on the habitually used right side and inability to spontaneously use the left capable side.* The right side would at once fumble and distract hopelessly. A careful study demonstrated a *typical right-sided apraxia*, not merely dependent on defective recognition of the objects.

The *aphasia* was *purely motor* — orders were understood, also conversation.

In reading, only short sentences were understood, not only in German but also in French.

Spontaneous speech was limited to ja, jawohl, ach, ach Gott, ach Gott ja, au, ne. At the request to repeat words, only rarely, the sound 'a' was obtained; otherwise the patient bows instead, or once he gave the match-box several times (executive perplexity).

Movements of the head as a whole, of the tongue, or face were bilaterally ataxic; movements of the eyes free, except that an order to look to the right was slower than that to the left at first, and to other orders he occasionally substituted a movement upward of the eyes.

The responses to orders to use the *arms* were very characteristic. To the request, 'touch your nose,' he fumbled with the right, but when the right was held, he promptly and correctly reacted with the left. The request to touch the nose with the left hand was carried out, but with simultaneous fumbling of the right; and a request to repeat this with the right, led again to mere fumbling. 'Show the right hand with the left' — he fumbles and picks up an inkstand; with the left he had promptly shown the right hand. Occasionally he makes correct movements on the right, (such as putting on his hat), but as if per chance, as they could not be repeated.

Imitations of movements did not succeed any better than the reactions to order.

The legs showed the same trouble as the arms.

Movements to auditory stimuli: Pointing to the direction of sounds failed on the right.

Response to tactile stimuli. Tickling of right ear brought no response with the right hand. He did not pull out a pin stuck in his right thigh. On the left he showed prompt response.

When asked to select objects from a number lying before him, he picked up a cigar, before any distinct request was finished; then, when asked to take the keys, he again took the cigar; but on repetition, he picked up the keys, and bowed.

At times, while he still blundered with the right and held the cigar, he picked out the right key with the left. There evidently was *precocious response*, and further it was shown that when he made mistakes, they could be corrected on simple repetition of request, or even simply on sign of dissatisfaction, or without repetition, to the remark,—'do it with the left hand only,' which shows that the request was grasped. On days of fatigue, errors occurred also on the left.

To test the understanding of names for objects, the patient had to make a + or — sign with the left hand according to whether the name

given an object was correct or wrong. Other signs (knocking with the left foot for *no* and raising of the left arm for *yes*), were less successful.

The mistakes which occurred in this test of *choice* were determined by: (1) Especially high objects; (2) very exposed and prominent or near objects; (3) perseveration (*pseudoapraxia*); (4) proximity to the object called for; (5) there was no preference for any special object.

Erroneous projection and mere mistake in grasping was excluded. He always touched or grasped correctly any point or object where no choice was involved.

Writing and *drawing* were fair with the left, but in mirror writing; quite deficient, however, with the right, but not totally perplexed.

Actions in which he always succeeded well were: Gait, use of spoon, mastication and swallowing, buttoning (even with the eyes shut), at times spontaneously, but especially when the button was once grasped; pointing to large objects (window, stove, lamp); smoking (but he often puts the burning end of the cigar on the table); playing simple tunes on the piano. Dressing was rather difficult (he puts the right hand and arm into the sock). The use of the comb brought out plain *apraxia*; the tooth-brush was only rarely used correctly. When he had to use both arms, the left usually did well, but it was confused and completely side-tracked by the fumbling of the right. An attempt to telephone led to quite an absurd reaction. Such a condition naturally suggested a disturbance of *sensibility*. *Vision* was good, the perimetric field normal; nor was there any one-sided mind blindness (he recognizes objects without fixation). Color sense normal (sorts wool). Pictures are recognized, also the physician's photograph. Hearing, taste, and smell are intact; he points to the correct card.

The *tactile sense* on the *left side* is practically normal, beyond a slight indifference to pricks on the left hand.

On the *right side*, however, sensation of trunk and face is less affected than that of the extremities. Deep pricks and deep pressure are perceived; medium pricks are felt, but not as pain; medium touch is not felt on arm and leg; pricks at times elicit *slight motions of the finger* which remain unconscious; fine touch is not felt on the face. Localization is good on the left, quite defective on the right arm and leg. The error usually is passing trunkward, up to two feet from the leg. The temperature sense is dulled on the right. Weights

(match-box empty or with sand) are recognized but relatively underrated by the right hand. The position of the right arm can be imitated by the left with the eyes open, but not with eyes shut. Passive motions of the right arm are not followed by the left, while he can imitate attitudes and passive movements of the left leg with his left arm. Imitations and simultaneous execution of passive motions of the left side by the right side does not succeed (contrary to what Anton observed in three of his cases, *Z. f. Heilkunde*, 1893, Vol. 14). *These facts do not, however, prove that, with his eyes closed, the patient has no knowledge of the position and movements of the right extremities*, because he does indeed use his limbs correctly for many things. *Stereognostic perception* was very difficult to test. The patient handled the objects and palpated them like a normal person; but with eyes closed the attention drifted. To avoid this, Liepmann had two sets of objects; he put one into a bag and had the patient feel and point with the left to the same object before him. There were 14 mistakes in 17 trials. Another test was to have the patient say whether he had the same object in the bag that had been there in the previous test. As to *motor symptoms*, the left angle of the mouth was plainly paretic without electric alteration. The tongue was freely movable, but could not be protruded; only once just after L. pulled it out, the patient withdrew and immediately afterwards put it out again, but could not repeat this. All other movements are possible, but perplexed on the right. The reflexes were weak; no contractures; no disorder of sphincters.

The patient could play simple *tunes* on the piano; he sang tunes correctly, occasionally with an indication of correct vowels. *Games* (checkers and 'mill') were lost.

Time orientation was fair. The orientation concerning his own personality preserved. He knocked when the correct number of his age was mentioned, or he found it among other numbers, or wrote with the left. But he required constant urging.

The memory was tested as follows: Optic: He is shown the number 817 and finds it again among 8 numbers, three and ten minutes later.

Auditory: He is told the number 1813 and finds it again in ten minutes among 8 written numbers.

Tactile: He only remembers things felt with the left.

Passive motions (cross, etc.), remembered only on the left. Personal memory without demonstrable gap. Attention habitually and spontaneously very deficient, but easily roused and held on urging.

Numbers and calculation: With the left hand he picks out a special number of matches; but cannot put out 2×3 , or $8 - 5$. Yet he *can* show the results of these calculations on a small table of figures; and he can add numbers with 3 figures with the left hand in mirror writing. Hence a *slight indication of apraxia of the left hand*.

Emotionalism was very marked; he would shed tears at first, but showed more indifference later. He is appreciative of compliments about his son, and shows signs of shame on seeing a hole in his sock.

Spontaneous mental activity: With the paper in hand he does not seem to grasp things; but understands what is read and later he asks at times to have the paper read to him.

For illustrations the reader should look up the original.

In most cases of so-called apraxia defective recognition of the visual and auditory impressions accounts for the difficulty and perplexity of manipulation (Heilbronner). In this case this is excluded. There is *no* one-sided *mind-blindness*; moreover the patient also shows apraxia to *requests*—to make a fist, to point to his nose, and when asked to scratch himself. The condition in the patient is *not ataxia, but deficient adjustment to a purpose*; it is therefore distinct from what happens with ordinary loss of sensations of touch and position. The writing is neat, but paragraphic; movements are correct as such, but inappropriate. This is not 'cortical ataxia.' Other cases with cerebral loss of muscular sense have been found to be unable to move the side with the eyes closed. In the cases of Anton (with hemianopsia) and Bruns (Nietleben, 1898) the voluntary activity of the affected side was diminished, difficult, *and ataxic*. Closing of the eyes did not change the picture in our case. He even persisted in using the right hand first, and there was absolutely no palsy as would be the case where real hemianæsthesia of this extent had persisted several months. The whole condition is, indeed, most correctly called *isolation of one function from others*—a condition quite different from abolition, a condition of deeply-founded distraction. The right arm evidently uses many sensations in buttoning, etc., but the centers of the left arm cannot imitate the position of the right; it cannot get access to the impressions from the left hemisphere. This agrees also with the fact that the patient moves the finger or the knee when it is pricked, although he cannot localize correctly with the left hand.

In the analysis of the motor symptoms Liepmann distinguishes carefully between those activities which are always normal and those

which at times succeed under favorable circumstances. Those which are constantly preserved should undoubtedly be considered as having an intact mechanism; thus our knowledge of human pathology shows that for intact gait, the so-called leg center *must* be intact: innervation from the other hemisphere would not prevent the appearance of defect in some of the muscles of the leg. It is, however, obvious that some movements, as chewing, movements of the tongue in chewing, swallowing, the grip of an object once taken hold of, and buttoning a button once grasped, are possible; hence the motor area is obviously *sufficiently* intact. Orders to button and unbutton the coat are not always obeyed; that is, the response through the auditory impressions cannot always be obtained. Orders, such as 'clinch the fist,' are not responded to properly. Even showing him the fist does not help him to the reaction, but the feeling of an object in the fist gives him the start to clinch it. This is similar to the inability of the aphasic patient to repeat single words out of the so-called recurrent utterances. For instance, Gowers reports a case which could not repeat the word 'no,' but finally in a state of annoyance said 'I can't say no.' The actions which are possible under short-circuit, remain in our case, but collateral influences fail to bring the modifying adaptations.

In the literature of cerebral pathology we often hear the terms memory picture, picture of movements, etc., and the loss of these pictures as referring to the destruction of definite 'centers.' This is evidently an attempt at harmonizing facts of pathology of the nervous system with the facts of psychic alterations. In reality the two ideas are not identical. That which is conscious in us, concerning a simple movement or an action, does not at all represent all the *parts* which must be in activity to bring about the movement. And what is meant by 'memories' and 'concepts' in neuropathology is not a psychological fact, but merely a term for the lasting character which the nervous substance retains from previous actions. It is, therefore, better to speak of 'mechanism for motions and for activities,' and not synonymously of concepts or pictures, as if what is conscious were identical with the mechanisms themselves.

The psychic part is as a whole difficult to describe and analyze, and especially so in the aphasia. In the tests as to whether the patient recognized objects when feeling them in the bag, it was practically impossible to say whether the patient did have a sort of a limited remembrance of what he felt. All we know is that he could not *use* the remembrance even if at times he had it 'perchance.' The difficulties in choosing objects, especially the premature reaction and the

perseveration, are explained by a lack of attention to more appropriate responses; the same holds for the absence of correction of mistakes, and perhaps also for the tendency to grasp for objects and pictures, instead of merely pointing to them. But the exact extent of inner language is difficult to grasp if not beyond reach.

For some motions the patient has double apraxia, namely, for the muscles which move the head, the face, and the tongue, and the movements he makes are not in harmony with ideas and affects; he is amimic or paramimic; he cannot '*look angry or cross*' to order. He cannot even *frown*, that is, use the muscle for orders which have nothing to do with emotion. *Amimia* has been observed by Perroud in connection with an apoplectic cyst of the third left frontal gyrus and the Island, and the neighboring marrow; this might be explained by the fact that the left hemisphere is the 'driving center.' In our case there is indeed a slight lesion of the right facial center.

The *mirror writing* is a defect of correction of the spontaneous and elementary symmetrical abduction. Simple mental weakness is not quite sufficient, since paralytics usually do not use mirror writing with the left hand.

For a *localizing diagnosis* Liepmann suggests lesion of the third frontal convolution and perhaps the Island, to explain the aphasia, and further a cutting off of the motor region of the extremities, especially from the auditory and the visual areas. This would be possible by a lesion in the parietal lobe. If it were as far back as the angular gyrus, reading would be destroyed and probably there would be hemianopsia owing to involvement of the optic radiation. A lesion in the marrow of the supramarginal gyrus and of the upper parietal lobule would, however, not necessarily have this effect. There is probably a small lesion in the right facial center accounting for the paralysis of the left angle of the mouth. Since the disorder of sensibility improved after the month of May (the apoplexy had occurred December 7, 1899), it is probable that the parietal lobule, as a whole, was not completely involved. The apraxia, however, did not change much through the recovery of the sensibility.

The picking out of objects became easier, but he could not put a bottle beside the keys on request; he could not clinch his fist, point to his nose; speech did not improve, only once he said 'it is all the same to me' (*ist mir egal*).

There was a slight improvement in the summer of 1900, then an apoplexy in October, producing right hemiplegia, rapidly improving on inunction. In December, even return of some indistinct speech.

January, 1901, again loss of speech, the right arm recovered, but was more ataxic; the leg paretic. End of 1901 the left hand too had partial apraxia. March, 1902, a third apoplectic attack with total left-sided hemiplegia without recovery and termination with pneumonia.

Anatomical findings: Extensive arteriosclerosis, especially of the basilar and the left Sylvian artery, depression of the left supramarginal gyri and upper parietal lobule; their convolutions atrophic but preserved; beneath them a large cyst, the posterior end of which reaches the anterior marrow of the angular gyri, but leaves intact the three layers of the sagittal marrow. The anterior central gyrus quite intact; the posterior nearly intact with the exception of a superficial bean-sized yellow patch and small superficial cyst; in the left island a small cyst; the Broca convolution very atrophic but free superficially. In the marrow of the left frontal lobe a degeneration of the thickness of a pencil. A small cyst in the rostrum of the corpus callosum; the entire callosum very atrophic. An approximal symmetrical similar focus in the *right* angular gyri involving marrow and cortex and a pea-sized focus of the internal capsule, perhaps the cause of the final left hemiplegia.

The terms asymbolia and apraxia have so far been rather hazily used to designate inadequate activity as distinguished from paralysis or simple ataxia. Meynert (*Clinical Lectures*, 1890, p. 271) had suggested the possibility of a separate existence of an essentially motor form with integrity of purely sensory memories, in distinction from the usual cases of secondary apraxia due to sensory defects. A third disorder, described as mind-palsy, reported by Nothnagel, Bruns, Anton and Bleuler, with a direct loss of definite motions, but without palsy, and also without apraxia, is to be kept apart. Liepmann's case is the first one of pure motor perplexity of the form of pure motor apraxia.

The case is a splendid demonstration of what degree of clearness a good and accurate knowledge of a patient's condition can bring. The patient was looked upon as demented, but through a careful examination an avenue for relations between the patient and the outside world was found to exist through the right hemisphere, while the left, driving or leading hemisphere was fumbling owing to isolation of its executive centers. Thus the case is not only one of the most interesting cases of cerebral pathology brought out during the last ten years, or perhaps since the discovery of the forms of aphasia, but a splendid illustration of the *practical* importance of accuracy.

Nothing but a complete translation would do Liepmann's excellent description and analysis justice. The original is certainly worth repeated perusal as a classic of neuropathology.

A. M.

Halbseitiges Delirium. PROF. BLEULER. Psychiatrisch-Neurologische Wochenschrift, No. 34, Nov. 22, 1902, p. 361.

An extremely interesting counterpart and addition to Liepmann's case, is this demonstration of the possibility of one side of the body enacting a delirium independent of the condition of the other side of the body, which may be relatively normal, or also engaged in a delirium, but of another character. Bleuler had a remarkable opportunity of observing a case of general paralysis who was lying in bed, perfectly calm with his left side, but in an active occupation delirium with his right arm: the hand seemed to grab for a rope, to chop things with an axe, sowing seeds (on question, the patient actually said he was sowing barley), or slinging away things with great effort as the face of the patient showed. The hand sometimes would get hold of the spread or the pillow and pull it away and go on in its delirium, while the left hand would again adjust the bed, wipe the mouth, and remain clearly in contact with reality. For a while the patient slept, but the right arm went on in its delirium. Touching, pricking and stroking of the left side was adequately appreciated and well localized; but on the right side it called for energetic fighting movements of an aggressive character, without any attempt to localize and without any participation from the left arm.

The right hand never grasped objects except by accident. The left hand grasped correctly but without doing anything unless he was asked to. Part of the time the right leg also participated in the delirium, apparently in coördination with the movements of the arm. For a short time both sides were in delirium; the right arm struck as if catching and throwing away small animals, or against a man, with a threat that he was going to pull his moustache; finally he shouted, 'now I have pulled it out,' and once, 'I have cut it off,' although he had only made pulling motions. With the left hand he independently made motions as if he were defending himself against wasps attacking the left side of the head, evidently something that he felt on the head; whereas, the right side delirium referred probably to things *seen* in hallucinations. Later, when his left hand was held, he was calm, when the right one was held he became violent. When spoken to from the left he turned calmly to the left; from the right side he responded violently, but probably only when touched. This condition was transitory.

No introspective information could be obtained later. It seems, probable, however, that the two complexes of consciousness were simultaneous, that one was delirious and in command of the language; that the other was not delirious, and occasionally with language at its disposal.

Considering the complexity of such a condition the observation is extremely suggestive and it will be well to be on the lookout for such conditions. An explanation is hardly to be expected for the time being. A. M.

Démence précoce. Anatomie pathologique et pathogénie. M. KLIPPEL ET LHERMITTE. *Revue de Psychiatrie*, Février, 1904, No. 2.

Four cases form the subject of the study. The clinical notes are very meager. The brains were examined especially for the size of the cell-bodies. One hundred large pyramidal cells of each of the motor zones and of the association centers were drawn and measured, so as to form means of comparison.

The first brain is small especially in the frontal region: the gyri are measured; the left central fissure shows a peculiar bifurcation into two deep sulci at its lower end. In the frontal cortex the cells of the normal size of 20 or 30 μ are rare, and are mostly replaced by much smaller cells of the same form; of 182 large pyramids only 31 measure 20 μ or over. A similar diminution holds for the temporal and occipital gyri, while the parietal cells are less affected and the motor region least.

The atrophied cells are rounded, with few and thin processes, granular protoplasm and marked pigmentation; the cells of the motor area are normal and have but rarely a small quantity of pigment. The small pyramids are nowhere affected. In the corpus striatum many cells show especially central chromatolysis and pigmentation, though less than the large pyramids of the association centers; many cells have from 6 to 8 satellites. Certain anterior horn cells of the oblongata are small and pigmented. Vessels and neuroglia normal.

The second case had a very simple convoluted brain with a few anomalies; the cortex measures from 3-3.5 mm. over the entire surface. In the occipital, temporal, parietal, and especially the frontal lobes the large pyramids show pronounced chromatolysis and pigmentation, in the frontal region often with dislocation of the nucleus and even splitting of the nucleolus. Near the vessels (not in the sheath) and around the cells there were numerous nuclei with almost invisible

protoplasm. The cells of the striatum are free, also the cord. No vascular changes.

The convolutions of the third brain are also small, without any diminution of the thickness of the cortex. A slight (evidently trivial) asymmetry of the cerebellum is mentioned. The distribution of atrophic pyramids is the same as in the other cases. The motor cells were varying but mostly large. There were no giant-cells of Betz, as seen in the other cases (obviously an accidental difference, as it is difficult to obtain identical regions). The motor cells of the ninth, tenth and twelfth nucleus much pigmented. No vascular changes. The liver with fatty degeneration.

The fourth case had first been suspected of general paralysis by some physicians. The diagnosis was, however, corroborated by the autopsy. The convolutions were generally atrophic; a marked asymmetry of the cerebellum is shown by measurements and in a drawing (as there is absolutely no explanation in the histological examination, very probably the distortion in hardening so frequently seen?). The findings in the brain were exactly as in the other three cases.

The absence of vascular lesions leaves the peculiar cell-changes, which may be:

1. Congenital anomalies, pointing to hereditary taint;
2. The essential changes, atrophy of the 'neurone' with precocious pigmentary granulation;
3. After-effects as shown in a lack of later development.

The writers point seriously to the cerebellar asymmetry, as the brain was that of a male, and since 'one knows how important the normal progress of puberty is for the development of the cerebellum.' The atrophy of the nerve-cells (only the cell-bodies are measured; yet the writers speak of atrophy of the 'neurone,' just as Bethe claims is usually done) is considered both direct and as an arrest of development. It is limited to the association centers. A fourth type of changes from the intercurrent disease is spoken of (liver, etc.).

In the discussion of pathogenesis the cellular disaggregation through destruction of dendrites is claimed as the basis of all dementia; in idiocy, dementia præcox, and in the adult. The absence of any other changes but those of the nerve-cells points to a functional constitutional selection. It might depend either on a specific cause or on an abnormal vulnerability of the 'neurone.'

The latter might be the result of a long series of weak but repeated pathogenic actions, involving both the heredity and previous diseases of the patient. The distinguishing of the two rests with the impor-

tance of the nature of the pathogenic agent itself, if it were a specific cause, and the mode of activity where it merely aggravates the vulnerability.

The haziness of the etiological factors in insanity comes out in the discussion of Morel's concept of degeneracy, which should in part be questioned owing to the fact that many writers find cases in previously normal individuals without hereditary taint. In order to preserve the concept a distinction is made between *dégénérescence immédiate* and *évolutive*; infectious diseases, alcoholism, intellectual and physical over-work, which during puberty lead to auto-intoxication; the characteristic feature of the condition evidently is the *mode of action* of the cause which ends in vulnerability and atrophy of the neurone with only casual proliferation of neuroglia, and without vascular disturbance. Thus, the point of attack of the anatomical process gives us a valuable interpretation; in organic dementias the tissue as a whole becomes diseased, but where the same causes (?) operate but slightly, but repeatedly, that is to say, when the *form of action* is mitigated, such a pure functional atrophy is produced as in dementia præcox.

He leaves open this possibility that dementia præcox comprehends all kinds of disorders and is an artificial complex.

A. M.

Un metodo de coloración selectiva del reticulo protoplasmático y sus efectos en los diversos organos nerviosos. S. RAMON Y CAJAL. 'Trabajos del laboratorio de investigaciones biologicas de la Universidad de Madrid,' Tomo II., Fasc. 4, 1903.

This study would seem to take out of the hands of the opponents of any kind of neurone theory the exclusive command of the situation concerning fibril stain in the nervous system; with a very simple process which gives fairly uniform results, Cajal has obtained pictures which lead him to uphold strongly his theory of the anatomical individuality of the neurone—that of contact and of polarization. With marvelous persistency he has examined practically all the cell types, and even invertebrates and developmental stages. The fibrils in most cells are not independent but form a net.

It is probable that the correlation of the results of this method with the results of the method of Bethe will add a new field of contentions to the already vexed problem, and it will be necessary to collect a wide experience with all the available methods, but no *exclusive* generalizations are to be attempted.

A. M.

Quelques considérations sur la théorie du Neurone. J. DEJERINE.
Revue Neurologique, No. 5, March, 1904, p. 205.

The practical value of some such simple statement of facts as the neurone concept allows, induces Dejerine to defend the term. For him it stands more on ground of experimental and anatomo-pathological than of histological viewpoints. But he acknowledges Cajal's recent reinstatement of his views on ground of his new method. The great point of attack is directed against Bethe's rejuvenation of the views of Philippeaux and Vulpian (1859). He emphasizes Vulpian's final declaration (1874), according to which the occasional regeneration of nerve trunks apparently permanently severed from the central nervous system must be attributed to recurrent fibers of peripheral plexuses.

In a case of Durante's of excision of a tumor of the median nerve, with death four years after the operation, a partial regeneration was found in the peripheral part of the severed nerve; since sensibility had persisted after the operation in the entire territory of the nerve Dejerine has good evidence of an atypical peripheral anastomosis in this case. There is, however, hardly any doubt that Bethe's demonstration that the differences concerning the autogenetic regeneration in peripheral nerves depends on the age of the animal, is better in harmony with the facts than such an explanation which runs counter to so many careful experiments.

Dejerine is on firmer ground where he speaks of the pyramidal tracts. He shows that the lesion is always limited to the pyramidal neurones and never involves an atrophy of the anterior horns. The latter are also exempt in the degeneration of the afferent reflex collaterals in locomotor ataxia.

There is no reason why the truths established in so many directions should lead to the exclusion of what good there is in the views of Apáthy and Bethe. That which occurs in regeneration in young animals is an event under abnormal circumstances, and the new situation may well be met on its own laws. Such findings as are offered by the regeneration process in the nervous tissue and other organs are bound to be extremely instructive. Like those of teratology they force us to broaden our conceptions of organization; they present dissociations worth knowing; but they do not necessarily disprove such generalizations as a non-dogmatic neurone conception represents in neurology and neuro-pathology.

The questions involved are extremely complex, and it is very difficult to get the individual observers to take all the facts into consid-

eration. Work in specialized fields invites narrower definitions justified for that field; but these narrower definitions should not give rise to dogmatic objections in larger spheres.

A. M.

BOOKS RECEIVED FROM MAY 7 TO JUNE 7.

- L'Année Biologique*. Y. DELAGE. 7th Année, 1902. Paris, Schleicher Frères, 1903. Pp. xcii + 642. [We call attention again to this most excellent annual. It is of especial value to psychologists both because it presents an authoritative and accurate résumé of the year's biological advance, and also because of its full treatment of psychological work (under the heading, *Functions Mentales*, edited by M. J. Philippe) — in this issue 120 pp.]
- Der Ausdruck der Gemüthsbewegungen des Menschen*. H. Rudolph. Textband and Atlas. Dresden, Kähmann, 1903. Pp. xiv + 228. [Atlas not received.]
- Memoirs of a Child*. A. S. WINSTON. New York, Longmans, 1903. Pp. 169.
- La Vision*. J. P. NAEL. Bibl. Intn. de Psychol. Dep. Paris, Doin, 1904. Pp. 376. 4fr.
- Die Ausstellung von experimental-psychologischen Apparaten und Methoden bei dem Kongress, etc. Giessen 18-21 April, 1904*. SOMMER. Leipzig, Barth, 1904. Pp. 78. [A descriptive and illustrated list of apparatus and arrangements as exhibited and demonstrated at the Giessen Congress.]
- Wissenschaftliche Beilage zum sechzensten Jahresbericht (1903) der philosophischen Gesellschaft an der Universität zu Wien*. Leipzig, Barth, 1903. Pp. 139. [Papers by Twardowski and others on *Das Wesen der Begriffe*, by Gorstel on *Die Axiome der Geometrie*, by Menzel on *Natur- und Kulturwissenschaft*, and by Urbantschitsch on *Die Beeinflussung subjectiver Gesichtsempfindung* this last having a table in colors showing the modification of the colors of after-images and subjective images in consequence of accompanying tactual and other sense-stimulation: a longer report of the experiments is to be found in *Pflüger's Archiv*, Bd. 94, 1903.]
- Der Ursprung der Kunst*. T. KIRN. German translation by M. BARTH; preface by P. PARTH. Leipzig, Barth, 1904. Pp. viii + 338. [A German rendering of Kirn's excellent *Origins of Art*.]

- Maine de Biran's Philosophy of Will.* N. E. TRUMAN. Cornell Studies in Philosophy, No. 5. New York, Macmillans, 1904. Pp. 93.
- Principia Ethica.* G. E. MOORE. Cambridge (England), University Press. 1903. Pp. xxvii + 232. 7/6.
- Essai sur l'Esprit Musical.* L. DAURIAC. Paris, Alcan, 1904. Pp. v + 304. Fr. 5.
- La fonction de la Mémoire et la Souvenir affectif.* FR. PAULHAN. Paris, Alcan, 1904. Pp. 177. 2 fr. 50.
- Grundzüge der physiologische Psychologie.* W. WUNDT. Fifth enlarged edition in three volumes. Bd. III. Pp. ix + 769. With separate Gesamtregister by W. WIRTH, pp. 133. Leipzig, Engelmann, 1903. Mk. 40 the set.
- Leitfaden der Psychologie.* TH. LIPPS. Leipzig, Engelmann, 1903. Pp. ix + 349. Mk. 8.

NOTES AND NEWS.

THE semiannual meeting of the Northwestern branch of the American Psychological Association met at the University of Chicago, Saturday, May 7, A. W. Moore presiding. The following papers were read and discussed: 'Image or Sensation?' W. C. Gore; Report on recent neurological work, H. H. Donaldson; 'An illustration of Psychology as Metaphysical Method,' S. F. MacLennan; Report of Experiments on relations between Sensations of Taste and Smell, Matilda Castro. After the program the association dined at the Quadrangle Club. Professors Scott, Tufts, and Tawney were chosen as the committee in charge of the next meeting which is to be held at Northwestern University on Saturday following Thanksgiving.

PROFESSOR JAMES H. TUFTS has been appointed to the headship of the department of philosophy in the University of Chicago, vice Professor John Dewey resigned.

PROFESSOR JAMES R. ANGELL has been appointed to a newly formed department of psychology in the University of Chicago.

The following items are taken from the press:

PROFESSOR JOHN G. HIBBEN, of the Princeton University logic department, has accepted an invitation to become editor of a new philosophical series, to be published by Charles Scribner's Sons. The series will be known as 'The Epochs of Philosophy,' and will con-

sist of twelve volumes, six of them to be written by leading American philosophers and six selected from prominent English and Scotch authorities. Dr. Hibben sails for Europe on June 7, to make arrangements for the foreign contributors, but will return to Princeton for his college duties next fall. He will himself contribute to the series a volume on the eighteenth century, to be entitled 'The Philosophy of Enlightenment.'

THE Woods Hole Biological Laboratory announces the revival of the *Journal of Morphology* (Vol. XVIII.) and also the establishment of a *Journal of General Biology*, which latter it would appear will cover largely the same ground as the newly established *Journal of Experimental Zoology* and also the British journal *Biometrika*. The question is suggested whether this duplication of journals may not be an indirect result of the expenditure of the funds of the Carnegie Institution.

As a result of the Congress held at Giessen in April, a German Association for Experimental Psychology has been formed, to arrange for meetings and also to undertake coöperative research.

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ZEITSCHRIFT FÜR PSYCHOLOGIE UND PHYSIOLOGIE DER SINNESORGANE, XXXV., 1. Über die Sehscharfe im Flimmerlicht: *H. Feilchenfeld*. Über die einfachen Reaktionszeiten der taktilen Belastungsempfindung: *K. Kiesow*. Beitrag zur Frage der Parosmie: *Beyer*. Miscellany. XXXIV., 3, 4. Das Mitgefühl: *B. Groethuyzen*. Über das Verhalten der Netzhautzapfen bei Dunkeladaptation des Auges: *W. A. Nagel and K. L. Schaefer*. Einige Beobachtungen über die Wirkung des Druckes und des galvanischen Stromes auf das dunkel adaptierte Auge: *W. A. Nagel*. Über die Wahrnehmung der Blutbewegung in den Netzhautkapillaren: *G. Abelsdorff und W. A. Nagel*. Miscellany.

INTERNATIONAL JOURNAL OF ETHICS, XIV., 3. The Problem of Teleology: *Felix Adler*. The Ethics of Passive Resistance: *J. C. James*. The Development of a People: *W. E. Burghardt DuBois*. Is Vivisection Justifiable? *C. S. Myers*. Professor William James' Interpretation of Religious Experience: *James H. Leuba*. Wordsworth's Ideal of Early Education: *John H. Muirhead*. What Should be the Attitude of Teachers of Philosophy Towards Religion? A Reply: *J. Clark Murray*. Byron Versus Spenser: *J. Kindon*. Book Reviews.

REVUE DE MÉTAPHYSIQUE ET DE MORALE, XII., 2. Les derniers entretiens, publiés par Louis Prat: *Charles Renouvier*. Ce que la médecine expérimentale doit à la philosophie: *F. Colonna d'Istria*. Les principes des mathématiques.—II. L'idée de nombre.—III. L'idée d'ordre: *L. Couturat*. La Raison et les Antinomies, III.: *F. Evellin*. Le 'devoir-faire' et le 'devoir': *A. Fouillée*. L'idée de Patrie: *P. Lacombe*. Miscellany.

JOURNAL OF NERVOUS AND MENTAL DISEASE, XXXI., 4. A case of erythromelalgia: *Arthur S. Hamilton*. Astereognosis in cerebral affections: *William N. Bullard*. Fracture of the Basis Cranii: *O. Waterman and B. Pollack*. Miscellany.

THE PEDAGOGICAL SEMINARY, XI., 1. Editorial. The Cat and the Child: *C. E. Browne*. How Words get Meaning: *Will Grant Chambers*. An Introductory Study of the Psychology of Foods: *Sanford Bell*. A Glimpse at a Nature School: *Cephas Guillet*. Literature.

THE PSYCHOLOGICAL REVIEW, XI., 2. Theory and Practice: President's Address: *William Lowe Bryan*. On the Attributes of the Sensations: *Max Meyer*. An Inquiry into the Nature of Hallucination, II.: *Boris Sidis*. The Mechanism of Imitation: *F. C. French*.

JOURNAL OF PHILOSOPHY, PSYCHOLOGY AND SCIENTIFIC METHODS, I., 5. The Relation of the Science of Religion to the Truth of Religious Belief: *A. K. Rogers*. The Concept of Consciousness: *Hartley B. Alexander*. Discussion. A Deterministic Analysis of Free Will: *C. A. Strong*. Miscellany. I., 6. A Criticism of Scientific Method as Applied by Sociologists: *Alban D. Sorensen*. Discussion. Pragmatism: *J. A. Leighton*. Miscellany. I., 7. Recent Philosophical Procedure with Reference to Science: *Ralph Barton Perry*. Notes Upon Logical Topics, II. The Meanings of the Term Idea: *John Dewey*. Discussion. A reply to Doctor Spaulding: *Walter T. Marvin*. Miscellany. I., 8. The Social Standpoint: *James H. Tufts*. Recent Literature on Scholastic Philosophy: *William Turner*. Discussion. The Standpoint of Instrumental Logic. *A. K. Rogers*. Miscellany.

ARCHIV FÜR GESCHICHTE DER PHILOSOPHIE, XVII., 3. Hobbes-Analekten. Ein bisher falschlich Locke zugeschriebener Aufsatz Shaftesburys: *Paul Ziertmann*. Ueber die Spuren einer doppelten Redaktion des platonischen Theaetets: *Alessandro Chiappelli*. Sur une erreur mathématique de Descartes: *Paul Tannery*. Die beiden Bacon: *A. Döring*. Locke, eine kritische Untersuchung der Ideen

des Liberalismus und des Ursprungs nationalökonomischer Anschauungsformen: *Georg Jaeger*. Die Lehre von der Bildung des Universums bei Descartes in ihrer geschichtlichen Bedeutung: *A. Hoffman*. Miscellany.

VIERTELJAHRSSCHRIFT FÜR WISSENSCHAFTLICHE PHILOSOPHIE UND SOZIOLOGIE, XXVIII., I. Egoismus und Altruismus. I.: *Demetrius Gusti*. Schopenhauer über die wissenschaftliche Philosophie. I.: *Cay von Brockdorff*. Ueber die Entwicklung des Begriffes der höheren arithmologischen Gesetzmässigkeit in Natur- und Geisteswissenschaften: *W. G. Alexejeff*. Miscellany.

THE MONIST, IV., 3. The Christ of Primitive Christian Faith. In the light of religio-historical Criticism: *Otto Pfeiderer*. The Coming Scientific Morality: *George Gore*. The Principle of the Conservation of Energy. From the point of view of Mach's phenomenological conception of nature: *Hans Kleinpeter*. Madame Blavatsky: *Henry Ridgely Evans*. Psychology on the 'New Thought' Movement: *John H. Noble*. The Élite of Democracy: *N. Vaschide* and *G. Binet-Valmer*. Miscellany.

MIND, XIII., 50. Professor Bain's Philosophy: *William J. Davidson*. Hegel's Treatment of the Categories of Quantity: *J. E. McTaggart*. Meinong's Theory of Complexes and Assumptions (I.): *B. Russell*. The Use and Abuse of Final Causes: *G. E. Underhill*. The Psychological Meaning of Clearness: *I. M. Bentley*. Miscellany.

